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The Growth of the Mind

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The Growth of the Mind

An Introduction to Child-Psychology

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TRANSLATOR'S NOTE

The hypothesis of the *Gestalt-Psychologie*, which the Author has here employed in elucidating the problems of childhood and mental growth, is for the most part new to English readers. Yet the Author's skill in executing his task, and his mastery of the results thus far achieved by experimental work in the field of child-psychology, have been so happily combined as to ensure a lively interest in his book. It is therefore gratifying to me, personally, to have been instrumental in making the book available to English and American students of Educational Psychology. Not only are many obscure points in educational theory and practice clarified by the fresh treatment they have received at the hands of the Author, but many attractive lines of investigation are suggested which students of Experimental Education will not be slow to seize upon.

In presenting the volume to English readers I desire to acknowledge the debt I owe my collaborators, Mr. Arthur W. Gilbert, Mr. Desmond S. Powell, and Dr. Seth Wakeman, for without their willing co-operation it would have been impossible for me to undertake the task of translation.

ROBERT MORRIS OGDEN

ITHACA, *March* 21, 1924.

AUTHOR'S PREFACE TO THE ENGLISH EDITION

WHEN I was invited to write a new treatise upon the Psychology of Childhood, the aim of the book, as it developed in my mind, was twofold. In the first place, I felt that I might be able to give a new and wider application to certain principles of psychological theory and research which have recently been advanced under the name of the *Gestalt-Theorie*, and thus might demonstrate their significance in the interpretation of childhood. In the second place, it was my belief that the teachers in the German elementary schools, for whom the book was intended, were in need of a psychology both modern and alive to the problems of the Educator. I believed it of urgent necessity that the psychological instruction usually given to teachers should be so modified as to set aside certain antiquated notions which, instead of promoting educational aims, often pervert them to such an extent that psychology is apt to become totally disregarded in educational circles. I was bold enough to believe that our new psychological principles would serve this useful purpose.

And hence what I have tried to write is not so much a compendium of facts as an exposition of principles In short, I have tried to work out some of the chief principles of genetic or comparative psychology, laying special emphasis upon the evolution of the child's mind. By keeping this object in view I hoped to

avoid a rivalry with two other recent works upon Child-Psychology by William Stern and Karl Bühler.

Yet I have addressed myself not to teachers alone, but also to my scientific colleagues, and to all students of psychology. In so doing I have not always found it easy to satisfy the claims of any one group of readers. To some the book will in many places seem too elementary, while to others certain passages, at least, will seem much too difficult. The first objection may be readily overcome by simply skipping the elementary passages. But the second is not so readily set aside; for true scientific knowledge can not be taken in like a spoonful of honey; it can be acquired only by intellectual effort, just as science itself is advanced only by strenuous research. For this reason a mere collection of scientific facts is never a true presentation of the scientific spirit and insight. To obtain these one must comprehend how the facts have been discovered, and what position they occupy in the comprehensive system of scientific knowledge. The principles upon which this knowledge rests should therefore be treated exhaustively, even though, in the end, they should have to be given up as false or barren; for otherwise the reader would not know why these principles can not persist, wherein their weakness lies, and how the explanation of the facts can now be bettered. In writing this book nothing has been further from my thought than a wish to engage in polemic for its own sake. The criticism of divergent opinions has been undertaken solely in order that the reader might become acquainted at first hand with the way a science like psychology has grown and is growing. Every science grows amidst a lively conflict as to the precise

nature of its foundations, and in this struggle the present book is intended to take its place.

I have adopted the plan of gathering all the notes together at the end of the volume, so that the text might have a rounded form, and also that the reader might not be disturbed in following a train of thought. In addition to references, a list of the notes contain a series of supplementary comments.

Though I have made use of abbreviations, I may here remark that to indicate age I have employed the method suggested by the Sterns in 1907, which has since come into general use: for example, 2.10 means the age of 2 years and 10 months.

The text and notes will give evidence of the unusual debt I owe to the available works on child-psychology. But since it is impossible to refer specifically to the source of every inspiration that has come to me, I wish here to make general acknowledgment of my indebtedness.

The translation of the book, for which I am greatly indebted to my friend Professor Robert M. Ogden of Cornell University, was a difficult task because of the new terminology employed, for which English equivalents had to be coined. The difficulty was increased by the fact that one of the chief terms employed, namely, *Struktur*, could not be retained as "structure," since, as a result of the controversy between *structuralism* and *functionalism*, this term has a very definite and quite different meaning in English and American psychology. For want of a better term, we have chosen to follow a suggestion originally made by Professor E. B. Titchener, and have translated *Struktur* as "configuration," although I can not say that it has completely satisfied me. This, however, is but one

of the many difficulties which have confronted the translator.

Since the publication of the German edition several important contributions to the topics treated in the book have either appeared in print or come for the first time to my hands. This new material I have endeavoured to work into the text so far as time has permitted.

K. KOFFKA

GIESSEN, *October* 18, 1923.

AUTHOR'S PREFACE TO THE SECOND EDITION

In preparing this new edition of *The Growth of the Mind* I have done what I could to improve the book under circumstances that were not altogether favourable. The first English edition was, I believe, an improvement upon the original German text. When, soon after its publication, I was asked to prepare a new German edition, I saw the need of many other improvements. Yet the time at my disposal was short, and not entirely available for that purpose. All I was able to do was to incorporate some newly discovered facts, and to revise the text so as to make it conform to my advanced views.

Upon invitation of the publishers to prepare a second English edition, Professor Ogden and I decided to insert the additions and alterations contained in the new German edition. Professor Ogden not only did this, but at the same time made a careful revision of the whole text. When I received the book in this form, a year had elapsed since the completion of the second German edition—a year rich in new contributions to my subject. This circumstance induced me to abandon the original plan. Instead of confining myself to a comparison of the English and German texts, I decided to introduce other additions and alterations. In the consummation of this task I take pleasure in

acknowledging the untiring and sympathetic aid which I received from Miss Beatrix Tudor-Hart.

When this work was completed, however, the publishers, who, in order to meet a steady demand for the book had made a new printing from the old plates, found it impossible to print a new edition until their old stock was more nearly exhausted. Thus the revised copy was in my hands when, in the late spring of last year, I learned that the time had arrived for resetting the book. But now another year had elapsed. Again new and important work had to be taken into account. Besides, I had several times given courses on the subject, and each time, I hope, I had corrected and clarified my views. I was now of opinion that some radical changes ought to be made in the arrangement of the book. But in order to make all the desirable changes it would have been necessary to postpone the publication of the new edition for at least a year. The idea, however, of submitting to my readers a book which was no longer the expression of my conception of the subject was averse to me. Therefore, I compromised by introducing such changes as I was capable of completing before I left the United States last August.

My main endeavour during the short period afforded me for revision has been directed toward making the book more consistent by the elimination of certain assumptions which were out of harmony with the general trend of the book. Especially with reference to concepts of inherited capacities and original tendencies, I found that I had taken far too much for granted in using these concepts in the first editions of the book. Furthermore, the work of Dr K. Lewin and his students has opened new vistas into psychological theory which

have deeply influenced my general point of view. Thus, the chapters on instinct and kindred topics have been extensively revised.

Of the other alterations I will mention but one. I have deleted several pages of discussion in the fourth chapter. A critic of the book has justly charged it with being too argumentative. I fear this criticism is still pertinent, but certainly it is less so than it was. I have tried in this edition to retain only such arguments as seem to have a direct bearing upon the fundamental questions of psychology.

This much had been accomplished last August when I thought that I had finished my part of the work of revision. When, however, I received the copy from Professor Ogden about a month ago for a last check-up, I took the opportunity of adding some new material and rearranging some of the old. This revision was completed in personal collaboration with my translator. And lastly, Professor Olive B. Gilchrist has been kind enough to read the whole book in its new form with a critical eye on the connections between the new and the old parts.

I regret that my work of revision could not have been more thorough. This book stands for a cause. It ought, therefore, to be as good a book as I can possibly make; otherwise the cause may appear to suffer on account of my deficiency. What this cause is every reader should know before he has turned the last page.

K. KOFFKA

NORTHAMPTON, MASS., *February* 1928.

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THE GROWTH OF THE MIND

CHAPTER I

PROBLEM AND METHOD

§ 1—*The Concept of Development in Psychology*

WHEN we set out to make a psychological study of the world in which we live, we continually come upon facts that can be understood only after we conceive them as products of evolution. For a long time psychological theory was dominated by the question: How much of any observed fact can be explained as a process of development? And even to-day no agreement has been reached between the rival theories of *empiricism* and *nativism*, the first of which emphasizes the influence of environment, and the second the influence of heredity. With this situation before us it is surprising to learn—though historically not difficult to understand—that psychology, and German psychology in particular, has made so little use of the general principles of development. Indeed, from the point of view of experience, the problem of development has been dealt with in a very specialized way, which is mechanistic rather than truly biological. This tendency seems, however, to be drawing to a close; for the need is now felt of introducing the facts of psychology into a larger sphere, embracing other facts of life, from which our science has already departed too far. We must therefore try to envisage the problems of mental growth as they really are; we must seek to understand the peculiarities of mental evolution, and must try to discover its laws.

In accomplishing this task we should not forget that the subject of a psychological investigation is usually the mature and cultured "West European" type of man; a living being—biologically considered—at the highest level of development. In the first place, we are dealing with the human being as opposed to the animal. Since Darwin's time, the conception of the descent of man has become common property, and we assume that what is valid in morphology and physiology must also have its significance in psychology. In the second place, we are dealing with representatives of a highly differentiated, as opposed to the members of a primitive, civilization. The world appears otherwise to us than it does to a negro in Central Africa, and otherwise than it did to Homer. We speak a different language from either, and this difference is a fundamental one, inasmuch as a real translation of their words into our own is impossible, because the categories of thought are different. In the third place, we deal with the adult as opposed to the child, though each of us was once a child, and has become an adult only by having outgrown his childhood.

We must not forget, then, that without a comparative psychology, without animal, folk-, and child-psychology, the experimental psychology of the human adult is and must remain defective. For this reason the psychology of the human adult has not infrequently and in various respects been unable to define its problems correctly, to say nothing of arriving at serviceable hypotheses. For instance, the error has often been committed of trying to explain a fact by merely referring to its evolution, thus building up a theory of evolution instead of first investigating the facts by comparative methods. Whenever one has had a genetic problem to deal with, the danger has always been great that one would accept the old hypotheses and apply them to his new facts, instead of first giving his facts an unprejudiced consideration.

We might think that in child-psychology the process of development would be obvious to every one; for we know the end-product to be an adult, with whom experimental psychology can deal, and the growth of the adult can be traced continuously from infancy. Yet this procedure is not so simple as it might seem; for as a matter of fact there is no principle of mental development which we owe directly to child-psychology[1] and, in so far as child-psychology makes use of any principles at all, they have originated either in experimental or in animal psychology. And yet there must be a genetic psychology; for the child-psychologist can follow the growth of a human being who in a relatively brief period of time changes from a simple inefficient individual into a highly complex and efficient man. It ought therefore to be possible to study this development in such a way that we can better understand the product, which is the human adult. Furthermore, if we could but understand this development, we should know more than we now know concerning the aims and methods of education.

This, therefore, is our problem: To discover the evolutionary principles of child-psychology. But although we must depend for assistance upon comparative psychology, we must not confine ourselves merely to transferring the principles of comparative psychology to our own field; instead, we must first test the value of these principles, and where necessary we must be ready to recast them.

§ 2—*A Provisional Consideration of the Problem of Psychology as applied to Child-Psychology. Mother and Child. The Observation of Events and of Conduct*

Let us now try to formulate the problems of child-psychology more precisely. As a provisional definition of psychology, we may say that its problem is the

scientific study of the behaviour of living creatures in their contact with the outer world. If we apply this definition to child-psychology, the thought immediately occurs to us that every mother is constantly doing just this; for no one knows the child so well, or understands his reactions and his impulses so thoroughly, as does his mother, by virtue of her unique and intimate relation to him. What need, then, of a child-psychology, if every mother knows her child better than the wisest psychologist can ever hope to know him? Without disputing this assumption the fact remains for us that psychology is scientific knowledge, since it employs a method which brings knowledge into conceptually formulated propositions. Psychology must have definite concepts; its statements are not made about "Infant X" or "Infant Y," but rather about those features of babyhood common to all ordinary infants. The mother may know that her child is now in such and such a mood, that he desires this, that in giving utterance to a certain sound he means a certain thing, etc.; but she can not transcribe her knowledge in scientific terms. In the first place, she usually knows nothing about scientific terms; and, as we shall soon see, if we wish to secure scientific knowledge a different attitude is requisite from that which the mother finds most natural. In order to become a scientist the mother must suddenly become an "observer"; she must tear herself away from the intimate relation in which she lives with her child, so that she may replace each intuitive bit of knowledge—unreasoned, though undoubtedly certain—by a critical analysis of the facts. She must therefore learn to distinguish her interpretation from the simple facts of the behaviour itself. But this implies that she must maintain a "distance" from her child, and must, at least during the period of any scientific observation, cut herself off from the intimacy of her maternal relationship with him. Mothers are naturally unsympathetic with this procedure, and have, indeed, a primitive

disinclination to allow their children to be thus practised upon by others. They can therefore be readily brought to oppose child-psychology in the fear that such observations and investigations may harm their children. On similar grounds an artist will often refuse to discuss theories of art. The mother has, of course, a right to protect her child from any injury that science might inflict, and in so doing she not only safeguards her child, but science as well; for an investigation which can injure the mental development of a child must almost certainly involve a wrong method of securing psychological knowledge. If one could reassure the mother on this point, much of her hesitation would disappear. Many mothers could even be won over to child-psychology, if it were made clear to them that they might thus benefit their children; for, although the mother's knowledge is intimate, it is, for the most part, a momentary knowledge, and if psychology could impart to her a knowledge of the chief characteristics of development, she would be far better able to guide and protect her child.

Furthermore, if the mother can be reconciled to child-psychology she can render an invaluable service. We have already pictured the procedure of the scientific worker in contrast with that of the mother; we must now emphasize the disadvantages of the scientist when he proceeds alone. The scientist has his ready-made concepts with the aid of which he seeks to understand the facts as he observes them. From the outset his gaze is directed through spectacles suited to his scientific view. Yet who knows but these glasses may be coloured or ground in such a way as to produce a badly distorted image? To give a concrete example: Since the child-psychologist has a genetic interest, he is inclined to regard each childlike expression, from an adult point of view, as an incomplete or preliminary step in the direction of a later and more mature end. Yet this view fails to note the individual significance

of the child's expression *per se*, which can not be seen at all through such glasses. Here the mother can and must assist. She knows her child from immediate personal experience, without preconceptions; she knows him and loves him as he appears to her at the time, nor does she ever try to think of her suckling babe as an immature college student. Each of the child's stages in development is of equal worth and importance to her, and she tries to understand each one of them in the same unprejudiced way. If she is successful in making her immediate knowledge available to others, she will have rendered the investigator a service not otherwise to be had; for she is able to furnish first-hand material which no scientific observer can obtain. This, to be sure, is a difficult task; for in its accomplishment she must be a good psychologist in the common-sense meaning of the word, just as a poet must be a good psychologist. A mother will then be able to teach the scientist how to observe naïvely; and nothing is more needed in psychology than naïve observations made by those who have an intimate acquaintance with their material, and are at the same time able to assume a critical attitude towards it.

But a mother's observations only supplement the work of the scientist. In order to understand the behaviour of a child in his contacts with his surroundings, one must undertake a great deal of troublesome special study, involving detachment and a thoroughly analytical attitude of mind. In this way it is possible to arrive at a view of the child "from without" by a method which I shall call the *observation of events*.[2] The essential thing here is to determine each single reaction-movement made by the infant. But what a child actually does can not be analysed into a mere sum of reactions; an adequate description of behaviour also involves the concept of conduct, and if we wish to achieve a "vital understanding" of the child's behaviour we must also employ a method which I

shall call the *observation of conduct.* In other words: In order to understand the child, we must know his reactions; but in order to understand his reactions, we must also know the child.

§ 3—*Functional and Descriptive Concepts. Three Kinds of Observation. The "Descriptive" Side of Behaviour*

We can now go a step further, and ask what is meant by the observation of conduct—which brings us to the problem of psychological method.

When we describe the behaviour of mankind, we use two kinds of concepts derived from three different sources. The difference can be made clear by a few simple, commonplace examples. I observe a wood-chopper, and find that the performance of his task gradually decreases without his giving me any impression of indolence. I can control this observation by determining how many blocks he splits in a minute, and from this I find that as the time is prolonged the number decreases. I attribute this phenomenon, this decrease in his efficiency, to *fatigue.*

Or, to take another example, I see a stranger lose something in the street, and I recover it for him. Next day I meet him again, and he greets me; that is, he reacts towards me otherwise to-day than he did yesterday, apparently as a consequence of yesterday's occurrence. I therefore say that he has *recognised* me, and I refer this fact to his *memory.*

Any one can reach these two conclusions concerning *fatigue* and the operations of *memory* who is able to observe these situations, for this is the general characteristic of a class of concepts where in any given case any one to whom the factual material is available will be able to decide whether a certain concept of the class is appropriate or not. We call this class of con-

cepts *functional concepts*, and they are of the same kind as all other natural-scientific concepts.

In order to acquaint ourselves with the second class of concepts we may again refer to our two examples. Whereas in the first example either I or any one else can determine the fatigue of the wood-chopper by his decreased efficiency, the wood-chopper himself is able to make quite a different observation. He may find, for instance, that at the beginning of his work, "It went easy," and that later "It went hard." Or he may say: "At first I felt fresh, but now at the end I feel tired." Likewise, the man who greets me in the street, thus leading me or anyone else present to infer an operation of his memory, may express himself by saying, "Your face, which yesterday was strange to me, now looks familiar."

These expressions attributed to the wood-chopper and to the man in the street are quite different in content, yet in contrast with observations of the first sort, made with the help of functional concepts, they have this in common, that the report of the wood-chopper can be made only by the wood-chopper, and the remark of the man in the street only by himself. No substitution is possible, for no one but the wood-chopper can say whether the work is tiring him or not, and no one but the man in the street can decide whether my features are familiar to him.

Facts which any one can determine are called *actual* or *real* things or processes. For instance, that the wood-chopper becomes fatigued, or that the person to whom I was yesterday a stranger now greets me, these are *real* processes. But we must also introduce a term for those facts which can be established only by a single person; these we shall call *experiences*, or *phenomena*. In order to prevent misunderstanding, it should at once be noted that the employment of these terms does not imply that experiences are unreal—that they are illusory or of inferior rank as compared with real

events. On the contrary, experiences have just as true an existence as do the processes we have chosen to call "real." In order to define *real* processes we have used *functional* concepts, whereas the concepts we apply to experiences we shall call *descriptive* concepts. In our examples, we have employed the descriptive concepts "feeling fresh," "feeling tired," "strange," and "familiar." We can also refer to the experience of freshness, of fatigue, of familiarity, of strangeness, or, to introduce a much-abused word, the *impression* of any of these. (2)

The consideration of this point may be carried a step further, because it is of especial importance to an understanding of psychology. To some, what has been said will seem obvious. Naturally, no one can get out of his own skin into the skin of another; my toothache does not hurt my neighbour, however much I might wish it upon him. But it may be remarked by others that there is something quite artificial in all this discussion; for if any one greets me he must, of course, know me, and I can readily assure myself of this without hearing what he has to say about it. In everyday life my assurance is that when one laughs he is gay, when one weeps he is sorrowful, and I can know all that without his telling me.

Both parties seem to be right, and yet they contradict each other, and so we may infer that perhaps the matter is not so simple after all. Of course it is true that in everyday life we act as though we could ourselves determine what kind of experiences another person is having, but we must not forget that in so doing we often fall into error, and sometimes are deceived by impostors. A person may weep, and arouse our sympathy, when the real cause of his weeping is not sorrow but onions. With absolute certainty all we are able to determine is the fact of his tears, but not how he may feel about them. Yet our behaviour in daily life is neither stupid nor without value. On the con-

trary, to become a good psychologist in the popular sense of the term is something to be highly prized. There is a way of observing real happenings, particularly in the actions of a living creature, which in a previous paragraph we have called the *observation of conduct.* Such observation is much more closely related to experience than the *observation of events*, and, when rightly applied, results in the formation of *functional* concepts which go much deeper into the nature of the processes observed. It must be added, however, that "conduct" is less certain than an "event," and must therefore be controlled by other forms of observation.

Turning to our examples, if the man in the street greets me to-day he must have recognized me, provided that one means by recognition a functional concept, a term to express certain operations of memory or conduct. In order to call his reaction a "greeting," I must have made an observation of conduct, because an observation of events would refer only to certain movements of his body. But that I appeared to him as some one he knew, who looked familiar to him, is a thing I can not be sure of from the mere fact of his greeting me; because it is also possible that, sunk in thought, or deep in conversation, he may have greeted me quite "automatically." Whether or not this was the case, he alone can say. Likewise in our first example, the investigation of the facts of fatigue has taught us that actual fatigue and "feeling tired" do not need to run parallel. And hence we must differentiate the two classes of functional and descriptive concepts according to the criteria of their application. For the first kind, any one, but for the second, only one person is in a position to decide whether the application in a certain instance is right or wrong.

We noted tentatively that the problem of psychology was the study of the behaviour of living beings in contact with their surroundings. Having discovered that psychology employs, not only the *observation of*

events, but also the *observation of conduct* and of *experience*, we can now make this definition more precise. Behaviour consists, not only of events, but also of conduct and experience. The psychologist's specific field of study is that of behaviour as conduct and experience. The one method of observation which is exclusively psychological, deals, not with the determination of real things and processes, but with experiences. We shall refer to this method as *experiential observation*, or perception, and thus avoid the commonly used, though unfortunately chosen, terms: "inner perception" and "introspection." To enter at this juncture on the very important but controversial problem of the perception of experience would be too much of a digression,[3] yet it should be noted that the method of perceiving experience is something that has to be learned and practised to an even higher degree than any other kind of scientific observation.

Def.

The best means of investigating facts with the aid of functional concepts are *measure* and *number*; mensuration and calculation can be understood or learned by any one. The concepts of the most highly developed natural science, Physics, are for this reason, quantitative concepts. Physical reports are always quantified, and it is the ideal of Physics to reduce all qualitative to quantitative differences.

The same can not be said of the facts of description, that is, of experience; for measurement is a typically functional method. Measuring with a scale supplies data that can be attained by any one. But in this sense experiences are not mensurable. Being only qualities, they seem to be at the opposite pole from the objects of pure Physics. The quantitative, in a natural sense, is altogether lacking in them.[4] Indeed this is the reason why the word "quality" is so often applled in psychology as though it were synonymous with "experience."

§ 4—*The Psychology of the Behaviourist. Criteria of Consciousness*

In opposition to this conception of psychology loud voices have latterly been raised, notably in America, where a tendency has arisen to set aside the differentiation which our theory of psychology accepts. It is the tendency to maintain that psychology is a natural science like any other, and, therefore, has no justification for the use of any peculiar method or of any distinctive facts. Consequently experiential observation and all descriptive concepts are banned, as likewise the "anthropomophoric" observation of conduct, leaving only the observation of events which are subject to general control. Behaviour being merely that which any one can observe and report of an individual, the psychologist need concern himself only with those reactions of an individual which can be determined by any one. The observation of experience affords no real data, because this method can not be controlled; a conclusion which seems to gain support from a wider view when our opponent insists that, biologically considered, man can not be separated from other living beings. And is it not, indeed, an error that traditional psychology should tend to concern itself exclusively with adult human beings, thus giving them a peculiar status, whereas man is but one of the many possible subjects of psychological investigation? In animal psychology one must necessarily do without descriptive concepts; for, since the animals are unable to communicate with us, no criteria of this sort can there be employed. Likewise in the psychology of early childhood, we can do no more than determine how the infant behaves under definite conditions. All else, being uncontrollable, must therefore be unscientific fantasy. If, then, normal psychology has no right to claim special privileges, it follows that we must limit ourselves to real facts, and translate the results of

psychology from the older terms of conscious content into the newer terms of behaviour. This means that, instead of reporting about experience, we may only admit reports about behaviour in certain situations where both the behaviour and the situation can be controlled by natural-scientific methods.

The advocates of this view call themselves *Behaviourists*, and, instead of psychology, they speak of the Science of Animal Behaviour or the Science of the Behaviour of Organisms. Since it is our purpose to treat also of Comparative Psychology, we must face this issue at once. In one important point the behaviourists are undoubtedly right. As soon as we leave the normal field of human adult psychology, the method of experiential observation has to be abandoned; no longer have we any criteria of experience, or any use for descriptive concepts. The mother may be ever so sure that her smiling baby is in a state of contentment; she may be able to read ever so clearly the beaming happiness in its face, but to a science that seeks its data exclusively in "events" these statements are uncontrollable, so long as they refer to the experience of the child. We can formulate this position by saying that outside of traditional psychology with its method of observing experience there are no criteria for the existence of consciousness.[5]

And yet attempts have often been made to find such criteria.[6] Two of the most important are these: First, it has been said that, so long as the behaviour of living beings can be explained in purely physiological terms, we should avoid the hypothesis of consciousness; this hypothesis being permissible only in case a purely physiological explanation is impossible. From our point of view such a procedure would be fundamentally wrong. Quite apart from the fact that there is no permanent criterion for such an inference—since a physiological explanation which to-day seems impossible may to-morrow be accepted—the hypothesis rests

upon the fallacy of supposing that a physiological explanation can ever be replaced by one of a psychological nature. To explain always means to determine the connections between facts, and to formulate the laws applicable to facts. Laws, however, are formulæ that can be controlled by any one; their objects must therefore, in the last analysis, be real things and processes. To explain the manifest behaviour of an organism by reference to an experience which can not be observed by any other person is to renounce all explanation in natural-scientific terms. We have already shown that, without some reservation, it is illegitimate to infer a phenomenal or conscious state from facts of a functional order. It may be equally fallacious to make an inverse inference from descriptive phenomena to functional processes. For example, in a certain investigation the observer reports that during the whole time he has steadily fixated a point without moving his eyes. What does this report signify to the experimenter? Only that the observer has had the same experience as if his eyes had remained unmoved; not, however, that no eye-movements have taken place; for whether the eyes have moved or not must be determined by the experimenter, and often enough he finds that, as a matter of fact, they have.[7]

At some point every so-called psychological explanation contains an inference of this sort. In comparative psychology, where experiential observation is lacking, fallacious inferences lead also from functional to descriptive concepts. The facts are easily obscured, because our language does not always possess separate words for descriptive and functional concepts. Our everyday concepts, of course, are not at all scientific. Many typically functional concepts are often called mental, and one forgets that the everyday meaning of "mental" applies to "conduct," and is not applied to what the psychologist means by "consciousness." *Intelligence*, for instance, is a "mental" term. One may say that

intelligence is requisite in such and such an achievement, and may conclude that the animal thus behaving must have been conscious. Here the error is quite patent. When one observes a performance which merits the term "intelligent"—such, for instance, as an appropriate discovery on the part of an animal—the inference is clear that the animal must possess a capacity for this achievement, and this capacity may quite properly be called intelligent. But it does not follow that the animal must therefore have been conscious of what it was about; nor is it permissible to call upon consciousness to furnish the explanation of an act of intelligence upon the assumption that without the aid of consciousness this act could not have taken place. One sees the disjunction of this argument in the passage from intelligent behaviour to conscious behaviour. From the facts of a certain observed activity I can not with any assurance infer what experience, if any, may have attached to it; and it is altogether without warrant to consider experiences as interrupting a chain of *real* processes. The behaviour of an animal as it takes place is something to be determined as a natural-scientific event. To explain this behaviour means to bring it into relation with other similarly conditioned natural-scientific events. So many observations must be made, and so many experiments performed, as are necessary to furnish the foundation for an assured inference, which, in principle at least, is always possible. To assume consciousness, however, and to refer the animal's achievement to it, are to abandon altogether the grounds of a scientific explanation.[8]

But the matter takes on quite a different aspect when one adopts the following point of view. In order to explain the animal's performance it may be necessary to assume brain-processes such as accompany what for us human beings are experiences—an assumption indicated especially by the observation of conduct. By

approaching the problem in this way it may be possible to justify the assertion of consciousness in the animal. At least the error is not committed of treating consciousness and functional processes on the same level; for the explanation remains in the realm of natural science. It must be admitted that we do not know what peculiarity it is that distinguishes those brain-processes which correlate with consciousness from any others, and hence this line of thought does not lead to an actual criterion of consciousness. But, even so, we may in time be able to bridge the gap between human and animal psychology if we continue to work with descriptive concepts in human psychology and practise observation of conduct in animal psychology. It is significant that the observation of conduct is being used more and more in the study of animal behaviour.

At least it is clear that we can draw no conclusions as to a criterion of consciousness by giving up the physiological explanation of behaviour; and a physiological explanation is obviously indicated for every mode of behaviour, even where a consciousness of the highest order is involved.

The second attempt which has been made to determine the existence of consciousness may be dismissed in a few words. It has been said that consciousness may be assumed wherever memory is involved in an animal's performance; but here again the fallacy of passing from functional to descriptive concepts is found in the same form in which we have discussed it with reference to the concept of intelligence.

§ 5—*A Denial of the Behaviourist's Point of View. The Significance of Descriptive Behaviour for Physiological Theory*

The behaviourist is right in denying the existence of conscious criteria wherever the method of experiential

observation is inapplicable; but in spite of this we shall refuse to accept this position, for the simple reason that there is a consciousness, reports of which can only be made by the experiencing individual, and which is therefore not subject to the control of others. Science can not refuse to evaluate factual material of any sort that is placed at its disposal. Furthermore, what appear to be two cases of the same objective behaviour may prove to be fundamentally different when the accompanying phenomena of consciousness are taken under consideration. A completely conscious action and an automatic action may seem to be identical, yet they may be widely different, while acts which are objectively quite different may be very similar when one considers the likeness of their attendant phenomena; and hence, were we to leave experiential observation out of account, we should often reach false conclusions. If the behaviourist answers that some natural-scientific method should be sought in investigating these differences, our rejoinder is that we are quite ready to leave that task to him; but at the same time the remark is permissible that it would never have occurred to him to search for such methods, had he not first become aware of these differences through his own conscious experience.

Finally, the bare fact that I am able to make a descriptive report is one of extraordinary significance. To me, it is at least as characteristic as that I breathe, or that I digest my food. A stick of wood can not do this, neither can an amœba; and when I am dead I shall no longer be able to do it. Were I not able to make a descriptive report of my behaviour, I should be unable to make any record of it at all. Paradoxically expressed, if one had only the capacity to make such responses as others can observe, no one would be able to observe anything.

It is therefore impossible to remove this aspect of behaviour from science, not merely because of its

immanent significance—since whatever we are, and of whatever we are proud, our culture, art and religion, would otherwise be incomprehensible—but also because of the intimate connection which experience has with the objective side of behaviour.[9]

The last point needs to be emphasized in order that what has been said may not be misunderstood. We have declined to accept a psychological explanation, and have advocated in its place a thoroughgoing physiological explanation, but we must nevertheless insist that our physiological hypotheses shall be appropriate to the complete behaviour of the organism, which includes also its experiential aspect. It follows that in the construction of functional concepts we must constantly give heed to the data of experience. Indeed, it is often our first task to secure accurate and significant descriptive concepts which are equivalent to a psychological theory. The formation of a new descriptive concept often leads to important consequences both in research and in theory, and as I have elsewhere shown [10] the criterion of a good descriptive concept is just this, that new facts and their functions are revealed by it. Functional adequacy always determines whether a new descriptive concept finds acceptance or rejection, a fact which in itself meets many of the objections raised by the behaviourist against the scientific evaluation of "facts" of experience.

In thus relating functional and descriptive concepts to each other we are only following the universal method of science. Yet we are making an assumption which should be explicitly understood, for in the relation between "outer" and "inner" behaviour—between conduct and experience as here conceived—the two are not "casually" linked together, but on the contrary are assumed to be *essentially akin*. Reverting to our earlier illustration of the wood-chopper, what we assume is that when the man feels tired and

his efficiency decreases, these two aspects of his behaviour are fundamentally united. Otherwise a feeling of freshness might as readily accompany fatigue as does the more natural state of feeling tired.[11] While this correspondence is not invariable, it will be found that functional and descriptive concepts coincide in their general aim and outcome, though they are less closely related in their origin. The importance of this general problem of correspondence is merely referred to at this point; but we shall later attend to one of its special aspects, and shall see how poor the behaviourist's equipment must be when he is not permitted to draw upon the full store of experiential facts.[12]

But, holding fast to "experience," do we not expose ourselves to the behaviourist's criticism of human psychology, that it is given a position quite apart from all other branches of psychology? We have already admitted that animal psychology has not supplied us with a criterion for the existence of consciousness. What, then, are the consequences to be drawn from this failure? We observe a dog whose master holds a morsel of food beyond the dog's reach. The animal assumes a very characteristic attitude, with its head stretched forward and upwards, the muscles of its body tense, and its ears pointed. We might continue the description in this manner, even supplementing it with pneumographic, sphygmographic, and other measurements. But is it forbidden us to summarize this description in a statement that the dog appears to be intent upon the hand of its master? Indeed, does not the enumeration of these details obtain its meaning from such a statement? Let us take another example from the work of Wolfgang Köhler upon the intelligence of chimpanzees.[13] In one place Köhler describes the affective expression of these apes. Referring to an outbreak of rage on the part of a female ape, Köhler writes: "We could sometimes observe her stamping

indignantly to and fro, throwing her head backwards and forwards, and not only shaking and clawing with her long arms in the direction of the scolder, but also seizing handfuls of grass and herbs, and tearing at them till the bits were strewn round her. If she had her blanket with her, she dashed it furiously on the ground, but always these gesticulations, both physically and psychically, were partially directed towards the enemy." Or again: "Under the influence of strong, unsatisfied emotion, the animal must do something in the spatial direction in which the object of his emotion is situated."[14] Köhler also observes that the same behaviour is characteristic of young children.

Descriptions of this sort do not merely tell us that an animal will throw things in a direction which is later found to be approximately that of its enemy; they show us, rather, that the animal is *directed upon its enemy*, and that every action arising from an emotion is controlled by this direction. Not only do the acts have this direction, but the animal is itself thus directed. No unprejudiced observer can doubt that a description of this sort is not only permissible, but desirable, and indeed necessary, in order to understand the animal's behaviour.

The behaviourist's argument can now be turned against himself; for, suppose we were to observe an outbreak of rage on the part of a negro in Central Africa whose speech we do not understand. Must we confine ourselves to an enumeration of details concerning his external behaviour? Are we not justified in saying that his anger was directed upon an object, upon a person? If we may and must say this, then we have grounds for denying that the psychology of man occupies a special position among the sciences, and are fully justified in describing a behaviour similar to that of man in the same terms we should use in describing man's own actions.

Conduct is the connecting link between human and

animal psychology. In confirmation of this judgment I may quote the following from E. C. Tolman, who calls himself a behaviourist: " 'A rat running a maze; a cat getting out of a puzzle-box; a man riding home to dinner; a beast of prey stalking its quarry; a child hiding from a stranger; a woman doing her washing or gossiping over the 'phone; a pupil marking a mental test sheet; a psychologist reciting a list of nonsense syllables; myself and my friend telling one another our thoughts and feelings.' These are behaviours. And it is to be noted that in mentioning no one of them have I referred to, or, I blush to confess it, for the most part even known, what were the exact muscles and glands, sensory nerves, and motor nerves involved. For these responses somehow had other sufficiently identifying properties of their own. And it is these other properties in which, as a behaviourist, I am interested."[15]

In describing conduct in this way we maintain that the animal's behaviour (both "inner" and "outer") is actually reproduced in these descriptions. In other words, we deny that a description of this sort endows the behaviour in question with mental properties which do not rightfully belong to it. Although a natural-scientific observation is commonly supposed to be strictly analytic, the application of strict analysis to an animal's conduct at once reduces it to mere mechanics of limb, and physiology of muscle and gland—a *reductio ad absurdum* which some of the younger behaviourists have realized. Yet the difficulty of maintaining a scientific point of view disappears when we allow ourselves to assume that animals possess certain characteristics of behaviour that can not be thus reduced to terms of analysis. It is freely admitted that this assumption carries with it very important implications for the whole theory of natural-scientific observation, which, unfortunately, we can not here pursue. But one of its outstanding implications is this: An essential

connection and a true correspondence to exist between our "total impression" of a certain type of behaviour and the real constitutents of the behaviour itself. The way in which we must conceive the nature of this connection, and the special conditions under which it becomes effective, are as yet unsolved problems, but they embrace the foundations of any adequate theory concerning our knowledge of the mental life of the "other man."

The validity of observations of conduct in comparative psychology must, of course, be ascertained; but the question of consciousness is of minor importance in animal psychology, because, as just remarked, consciousness, when present, is essentially like external behaviour, and this behaviour has the same characteristics of conduct whether it be accompanied by consciousness or not. Concepts derived from the observation of conduct, and hence functional in nature, are of the same order as descriptive concepts.[16] Accordingly, we need have no further anxiety about the application of descriptive concepts to animal behaviour. The statement, however, is not to be taken as a defense of the anthropomorphism common to the older animal psychology, which consisted more in pretty anecdotes than in scientific facts. To have made the attack upon this uncritical attitude is to the lasting credit of the American investigators; but they have gone too far, and in their desire to be "objective" they have relinquished much of their best material.

The same point of view that is valid in animal psychology is likewise valid in the psychology of childhood; for naturally the problem whether consciousness is present or not plays a much less important role in infancy than it does in animal behaviour. Only during the first days of life can the presence of consciousness be questioned; furthermore, another criterion aids us in certain cases to decide whether or not the infant is conscious.

§ 6—*Consciousness and the Nervous System*

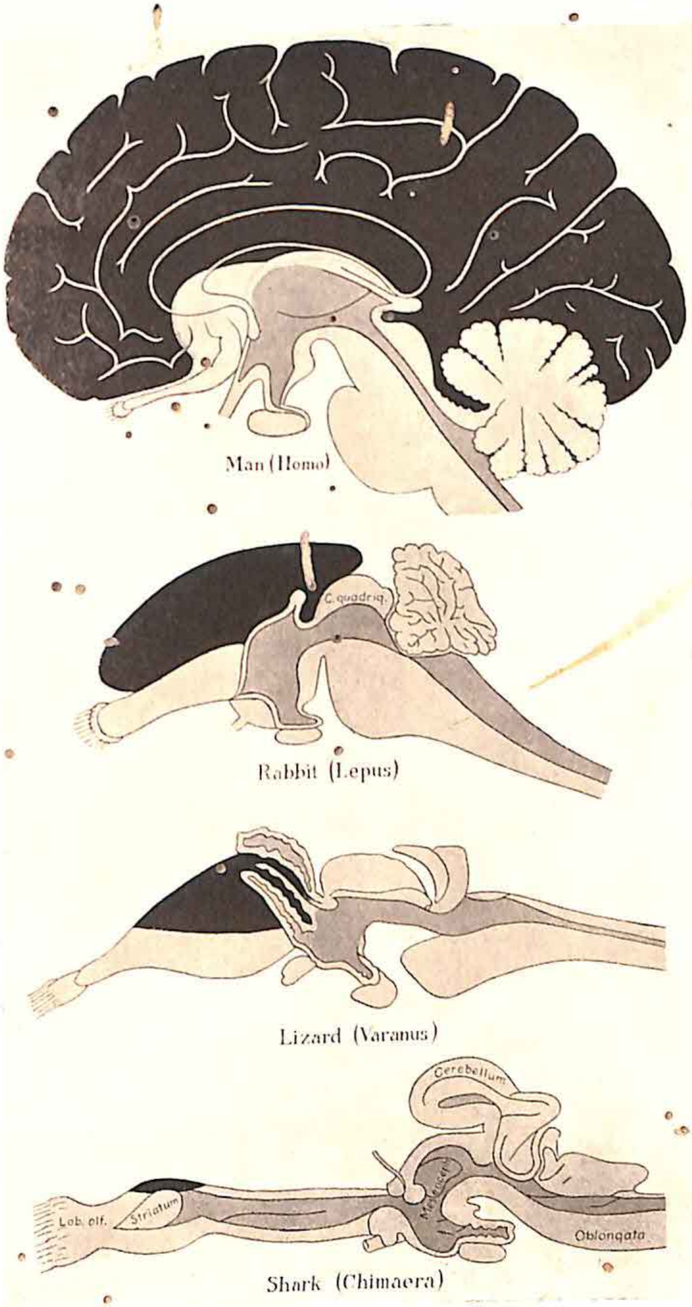
In order to understand the nature of this additional criterion we must first take a glance at the anatomy and physiology of the nervous system. The complete behaviour of the higher animals is controlled by their nervous systems. The central apparatus receives all the nervous pathways that make the reception of stimuli possible. This we call the central nervous system, which is stimulated both by processes that take place in the surrounding world and also by those of the organs of the body itself. The central system likewise issues in nervous pathways by means of which all movements are aroused. Processes of the first kind involve the sensory, afferent, or receptive nerves, their connection with the outer world taking place either in specially constructed organs called sense-organs, or else in the free nerve-endings of the skin. The second class are called motor or efferent nerves; which end in muscles or in glands, and thus control the bodily movements and secretions. Among the various parts of the central apparatus we are concerned only with the central nervous system, for we can not here enter upon a study of the autonomic system, whose importance, however, becomes every day more apparent.

According to Edinger [17] we can differentiate two parts of the central nervous system; one of these, found in all vertebrates,[18] fulfils the function of the central apparatus to which we have already referred, in that it receives sensory impulses, and sends out motor impulses. This apparatus consists of the long and extended spinal cord (*medulla spinalis*), which continues into the *medulla oblongata*, and also of a series of brain-parts among which the cerebellum, the hind brain, the mid-brain, and the olfactory lobes may be named. This organ, when taken altogether, is termed by Edinger, the "old" brain (*Palæ-encephalon*). To this original apparatus there is added, in the developmental series

from the shark upwards, a new apparatus, called the *cerebrum*, which constantly increases its size until in man the original apparatus is completely covered by it (see Fig. 1). Edinger calls this the "new" brain (*Ne-encephalon*). The "new" brain is in the closest connection with the "old" brain, receptive pathways leading from the "old" to the "new," where they terminate at the surface, or cerebral cortex. Motor pathways likewise lead from the cortex into the "old" brain, so that this later, yet far more effective, organ is capable of influencing the "old" brain, and thereby the behaviour of the entire organism.

We shall return to these matters again. For the present it is of interest to note that in man, an organism which, as we shall see, is more dependent than any other animal upon the functions of the cortex, those phases of his behaviour which take place through the functioning of the "old" brain alone, without any co-operation of the cortex, appear to be unconscious. Since the "old" brain gives rise to no experiences, man knows as little by way of it as he does of what is happening on the moon. A chance observation of Edinger furnishes us a crude illustration of this fact: "I observed the case of a woman in the act of labour, whose spinal cord, as a result of spinal caries, was totally incapable of carrying afferent impulses to the cortex. Consequently she went through all the characteristic movements of childbirth without in the least feeling these otherwise painful processes. Indeed, she discovered only by chance when some one came to the bed to render her assistance that she was in the act of giving birth. This patient has repeatedly assured me that she was altogether unconscious of this entirely palæ-encephalic reaction." [19]

If we were to assume the same dependence of consciousness upon the "new" brain of the suckling, we might infer that, if there is a period of time in which the infant behaves in a purely palæ-encephalic manner,



[*After* **Edinger**

Fig. 1. "OLD" BRAIN, GREY: "NEW" BRAIN, BLACK

[*face p.* 24

it is highly improbable that the child's behaviour is at that time accompanied by consciousness. In the course of our investigation of infancy we shall return to this question.

§ 7—*Division of the Psychological Methods*

Psychology employs objective methods—in the observation of events and of conduct—and subjective methods—in the observation of experience. According as these are employed singly or in combination we have three classes: (1) a purely objective method; (2) a combined objective-subjective method, called psychophysical; and (3) a purely subjective, descriptive method, depending altogether on the observation of experience.

1. The Objective Method consists in observing the individual in a certain situation. An experiment of this sort can readily be constructed by controlling the state of the organism—for instance, by depriving it of food—and likewise by controlling the situation in which observations are to be made. Oftentimes an experiment of this kind involves measurements; for example, in the investigation of fatigue one can determine the quantity of work accomplished in a given unit of time. Or, again, one can measure the time taken by an individual in the solution of a problem. Such experiments are often referred to as *achievement-tests*. In their emphasis upon measurement, these methods rest chiefly upon the observation of events. The importance of the observation of conduct in tests of achievement has, however, been demonstrated by Köhler's experiments with chimpanzees, which are described at length in Chapter IV.

2. The Psycho-Physical Method is distinguished from the first type in that a "description" of behaviour is also included as a part of its data. One includes not

only the data observed by the experimenter, but also those reported as being the experience of the observing subject. This method also is employed, for the most part, in the form of an experiment. The situation is controlled by the experimenter so far as possible in mensurable terms. The behaviour of the subject is then studied while the situation is being altered in a prearranged manner so as to provoke corresponding changes in behaviour, which is understood to include the experiences reported by the subject. The aim of this method varies according as the emphasis is placed upon the descriptive, or upon the functional data involved. This difference can be made clear by the following examples.

(*a*) The investigation of auditory perception may be referred to as emphasizing the descriptive aspect of behaviour. If I wish to find out what auditory experiences occur when an individual is stimulated by various kinds of sound, only the sound-processes are varied. These being the relevant factors in the situation, the procedure is much simplified. We call these variable elements of the situation, which have a bearing upon the experiences of the observer, *stimuli*, and they must be varied in a systematic way. For instance, the experimenter arranges simple sound-waves of variable frequency and intensity, and then replaces these with more complex waves. In short, such variations are introduced as may be necessary in the solution of his problem. It will at once be seen that, without the guidance of a descriptive point of view, the selection of an appropriate method in any psychological investigation is virtually impossible. That is why the description above is so vague, although it may suffice for the purpose at hand.

After hearing the sound, the observer proceeds to describe the effects of the different stimuli, and, generally speaking, this description involves certain kinds of behaviour. For instance, the observer may be

called upon to judge whether two tones are equal or different; in what respect and in what direction they vary, etc. These judgments involve acts of behaviour which can be determined by natural-scientific means. In fact, we do not need the observer's report at all, since it can be replaced by other reactions, such as we are obliged to introduce in the tests of animals. We can, for instance, *train* the individual to make a certain response whenever the higher of two tones is sounded. If the training is successful under conditions which make it possible to ascertain that the response was not based upon a difference of intensity, or some other factor than pitch, we may conclude that the frequency-number has been the effective agent in producing the observer's reaction. Yet this fact can be determined much more quickly, and much more easily, by simply asking the observer whether the two tones were alike or different.

It is, however, quite true that the report is only a convenient and abbreviated type of behaviour, and that in so far as the report refers exclusively to behaviour it can be replaced by a mensurable reaction. But, as our last example clearly shows, this in no way justifies the behaviourist in assuming that the observer's report is altogether negligible. The training-tests, introduced as a substitute for the psychological report, may indeed show that an organism is capable of reacting differently to two sound-waves of different frequencies. But this result, as we know, is psychologically insufficient. For instance, suppose I test two observers, A and B, with the tones of 500 and 600 v. d., and suppose that the test has in each case been successfully administered. If we examine the reports of A and B, A may say: "The two test-tones constituted a minor third, and I reacted to the higher"; while B may express himself quite differently—perhaps he does not know what a minor third is, nor when one tone is *higher* than another. Instead B may describe his experience by saying that

one tone was *duller*, and the other *brighter*, and that he reacted to the *brighter* tone. Although the training was successful, and the objective behaviour the same in both cases, yet so different are the descriptions given by these two observers that we must conclude that the results of the training involve different types of behaviour. In fact, tests are certain to show that observer A is much more capable of auditory training than observer B; yet, without knowing anything of their respective experiences, how could one find out wherein this difference lay, or upon what it depended?

If, on the other hand, an observer can master the descriptive aspects of the situation so as to be able to differentiate such attributes, for instance, as Köhler's "tone-body" and pitch, then tests of behaviour can be made which are calculated to determine the utility of these descriptive results. This example, furthermore, demonstrates that experiential observation is not so simple an affair, and that to be able to proceed from a certain observation of experience to the construction of an appropriate descriptive concept of it may itself be a highly significant performance. Köhler was the first to define the concept of "tone-body," as a description of certain auditory data already well-known to the psychologist, though never before formulated. Thus, sooner or later, inadequate descriptive concepts act as a check upon investigation; but progress can never be made, even with the aid of this check, by such a total abandonment of descriptive concepts as the behaviourist has proposed. Progress can and will come, however, with a continuous refinement of these concepts, as they are employed by investigators with constant reference to the overt response in connection with which the experience occurrs. Both the response and the experience must be intimately correlated, as, indeed, they always have been in the psycho-physical methods. Whenever we succeed in setting up a new and useful descriptive concept, it is immediately apparent that the multi-

plicity of relations between stimulus and behaviour (both external and descriptive) become more distinct and intelligible. The relation itself is a natural-scientific fact which can not be reported by an observer with his incomplete information, because the observer reports now this experience and now that. The experimenter accepts his report as something to be studied in connection with the nature of the stimulus which, as a rule, is not known to the observer. In this procedure the influence of the experimenter is negligible [20]; for after the results have been recorded any one can do the work of determining what uniformities they show, and likewise any one can criticize, and should be ready to criticize, the conclusions reached. The individuality of the observer, however, is always material, for we can not attribute to observer A an experience that has been reported by observer B.

The final outcome of the psycho-physical method is a law expressed in terms of a functional concept. But this outcome is not arrived at without the employment of descriptive concepts; and under certain circumstances the definition of a new descriptive concept may, indeed, be the most important result of a psycho-physical investigation.

(*b*) Emphasis upon the functional side of the psycho-physical method can be illustrated by the investigations of memory. A number of the important methods in the investigation of memory consist in impressing certain material (preferably nonsense-syllables in an ordered series) more or less firmly upon the observer, and, after a definite interval of time, determining by various methods what the observer still retains; how quickly he can reproduce it; what errors are made, etc. So far we are dealing merely with certain types of behaviour. But memory-experiments are more than tests of behaviour; for the observer is also asked to make reports upon his experience while learning. We ask him to describe the images he reproduces, and to

state the degree of certainty with which the reproductions occur, etc. The compilation of these reports permits a fuller understanding of his behaviour, which latter is more in focus here than it was in the investigations described under (*a*). Yet the principle is the same in both cases. The enormous significance of working the material over in the mind before it will be retained [21] can hardly be determined without the aid of descriptive data. Yet this comprehension of the material is a fundamental datum in any doctrine of memory.

3. The Purely Psychological Method renounces all claim to natural-scientific observation, and is satisfied with experiential observation alone. The method as such is of greater importance to the psychologist than it is to psychology; that is to say, a contemplation of psychological phenomena will often suggest to the psychologist that certain hypotheses which have been framed to embrace these phenomena are incorrect. The psychologist will then seek to test his hypotheses by other, and especially by psycho-physical, methods. For this reason the psychological method is not to be rejected, because it may be very useful as a beginning, or as a preparation for a scientific investigation, and it may even set new problems, and suggest new hypotheses, as well as lead to the formation of new descriptive concepts. On the other hand, we can never be entirely content with this method alone, since it always stands in need of a substantiation and a supplementation which can only be had by employing other methods.

§ 8—*Methods in Child-Psychology* [22]

In the psychology of childhood, and especially in the first stages of the child's development, the observation of behaviour plays a leading part; and not only in the pre-linguistic stage, but later also in the investigation of the linguistic performances themselves. With caution, one can infer the experiences of the child directly from

these performances. Révész has done so in a special article,[23] and Piaget uses this source of information in his studies of the child's phenomenal world. Already in discussing the question of consciousness we have seen that in a scientific understanding of the organism's objective behaviour, it may be of the greatest importance to be able to form a picture of what was being experienced while this behaviour was going on. We must therefore consider the psychological aspects of infantile behaviour, and be prepared to employ descriptive concepts without the aid of any direct report of the child's experience. In order to accomplish this end a " psychological talent " is requisite which shows itself in two ways. The first way is to try to put ourselves in the place of the child, with the same tasks before us which the child is expected to solve, and with only those means at our disposal which are available to the child. Thus we can endeavour to determine the characteristic phenomena occurring under these conditions.[24] As a working hypothesis we may therefore assume that similar phenomena are present in the mind of the child, though we have then to verify this hypothesis indirectly by achievement-tests. The second way is to employ the observation of conduct, which leads to a description of traits characteristic of both inner and outer behaviour. A mother with her intimate knowledge of her child will be most competent to give such descriptions of conduct. One should, however, constantly seek to control her statements.

Concretely, how must we proceed ?

1. Most of the knowledge we now have of early childhood we owe to diary-notes concerning the development of individual children. From the first days of the infant's life a mother, a father, or some one who is intimate with the child, observes what he does, and what happens to him. Needless to say, the child's natural development should be recorded as completely

as possible; but, strictly speaking, one can not record everything. A selection being necessary, all depends upon its appropriateness. In making his observations the observer must therefore assume a certain attitude. He must consider certain things, or his observations will be aimless, and many important matters will be overlooked. These diaries of child-life are therefore not uninfluenced by the character of the writer, by the problem with which he is concerned, and, indeed, by the level of his child-psychology. The diaries we have often give no answer to certain questions arising in the study of infantile development. These questions therefore lead us to begin new diaries intended to record data that will answer these questions. What I wish to say is only this, that the collection of data is not a merely mechanical and receptive affair, since the greatest foresight and the strictest self-criticism are demanded of those who keep diaries of child-life for scientific purposes. In the diary itself only actual observations should be recorded, and nothing at all in the way of interpretation.[25] This, however, is easier said than done; for, in order to describe a child's behaviour, concepts are needed, the applicability of which can often be decided only by recourse to the behaviour that is being described. Such concepts, for instance, are "environment" and "reaction." If one understands by environment, not the physical surroundings of the child, but rather his biological surroundings, and even, under certain conditions, his phenomenal surroundings, then the environment can be known only with reference to the reaction, and sometimes the reaction *qua* reaction can be understood only in relation to the environment. On the other hand, caution should not carry us so far that the observation of conduct is reduced to a minute observation of events.

2. The occasional observation of a noteworthy performance may also be valuable in the investigation of

infancy. But one must know the exact conditions under which it occurred. The record of such observations must therefore be very accurate, and should include a description of the complete status of the child as well as an account of the special conditions under which the behaviour took place.

3. At the time when this book was first written, experiment, which is the most important method of normal psychology, had not yet attained the position it should have in the investigation of childhood. The reason is clear when we consider that these experiments deal almost exclusively with achievement-tests which hitherto had fallen without the scope of experimental psychology, and for which no exact methods were then available. The methods employed by the American animal psychologists, which we shall discuss later, could not properly be applied to a child. To be sure, experiment had not been altogether lacking. J. Mark Baldwin carried on experiments with infants to whom he presented objects which varied stepwise in certain directions. For instance, colours were shown, and the observation was made as to which would be grasped spontaneously. Other investigators have undertaken experiments upon children, which were copied from the training-tests of animals. But, generally speaking, one can say that until the last few years experiments had not been adapted to the most important problems of child-psychology. The situation has now changed; interest in genetic psychology has revived, and a new and thoroughly experimental child-psychology has arisen.[26]

About ten years ago Köhler succeeded in devising tests suitable for the investigation of the most important problems of behaviour, which he has applied to anthropoid apes, and which can readily be carried over into child-psychology. Indeed, Köhler has already conducted some experiments with children,

and Bühler has followed him with others of a like nature.[27]

The chief condition which these experiments fulfil—and a condition which all good achievement-tests must fulfil—is that the demands of the investigation shall be accommodated to the level of the subject; so that he is not placed in situations entirely artificial and of necessity unintelligible to him. But in addition—and this is of the utmost importance in child-psychology—Köhler's tests are of such a nature that the normal and healthy development of the subject experimented upon is in no wise disturbed by them. We may confidently expect that with the aid of this new method, which at the proper time will be described in detail, child-psychology will make a great stride forward.

During the last few years there has been an enormous advance in the experimental investigation of children. In various parts of the world, and notably in the United States of America, Nursery Schools, Child Welfare Research Stations, and similar institutions have been founded, which afford new and enlarged opportunities for the scientific study of childhood. Although this situation is most fortunate, a note of warning, perhaps, will not be out of place. Frequently, in these new investigations, too much emphasis is laid upon mere factual chronology as determined by "batteries" of tests, while too little emphasis is placed upon the dynamic process of development, and the observation of conduct. A group of tests which will give negative results at the age of six months, let us say, and positive results at the age of one year, may not be quite worth the amount of time and effort required for its construction and application. The statistical method which is frequently, and sometimes exclusively, employed has its merits; but we should not forget that it is a method and not itself a result. Of itself, a curve of distribution gives little knowledge, however appropriate it may be as an indication of

some dynamic process. What is needed is a little less "measuring" and a little more thought and naïve observation.

In conclusion, let me refer to an investigation conducted by Alfred Binet. This investigator believed that one could replace the experiments upon children by experiments upon feeble-minded adults who might be considered as "stereotyped children" of a mensurable age, and who, just because of this stereotypy, ought to furnish precisely the kind of subjects needed for experimentation. But "no more than dwarfs can be considered children of suspended development, can the feeble-minded be compared mentally with certain ages of childhood."[28] For this reason alone Binet's method must be rejected as totally unsuited to the investigation of the mental development of children.

It is a different matter when one makes use of retarded children for the investigation of a definite problem, because a certain process may stand out more clearly in their behaviour than it does in that of normal children; since retarded children learn with greater difficulty, they remain unstable for longer periods of time, and they acquire automatic responses less quickly than normal children. For these reasons investigation is sometimes more effective with retarded individuals than with normal children, and an experiment of this sort undertaken by Peters has brought good results.

No general rules for the treatment of the results of observation and experiment can be laid down. Claparède emphasizes the importance of two questions: (1) What is the present developmental status of a certain type of behaviour? For example, does the child still merely babble, or does he understand his words? Suppose one has observed a certain "reaction," and wishes to know what its significance may be as an "achievement." This question leads to that much disputed and almost always misstated problem: Is the behaviour in question

inherited or acquired, or, more precisely, what part of it is inherited, and what part acquired? (2) What is the present function of the behaviour? For example, we must ask: What process performs the same function in a child of a given age that conceptual thinking does in a man? On the other hand, we should not ask whether a child thinks in terms of concepts; because we can not use the same procedure in approaching the mental life of a child that we are accustomed to employ with adults. In the first place, we know very little about the thought-processes of adults—much less, indeed, than our own philosophy would warrant us in supposing. Having originated in logic, the concepts with which we work—for good or ill as the case may be—have lost all connection with living thought. In the second place, by asking such a question we block the way to anything which may be specifically different from that which an adult might expect to find. Whenever an ethnologist of an earlier period was satisfied with ascertaining that a people could count only up to five, the nature of his question destroyed every possibility of securing insight into the processes of calculation which these people may have employed as a substitute for counting. Against this kind of error in child-psychology we can not be too much on our guard.

§ 9—*Books on Child-Psychology*

We shall mention here only a few of the more important books on child-psychology. A list of the works frequently used in this volume will be found preceding the notes in the appendix. The remaining citations are listed in the notes themselves, while ready reference is facilitated by the arrangement of the Index.

The standard book on child-psychology is the work of William Preyer, published in 1882. It is still a mine of observations, and is really indispensable, although in its theory it is long since out of date. A good charac-

terization of the work may be found in Bühler's book on the same subject.

W. Preyer, *The Mind of the Child* (translated by H. W. Brown). Part I. The Senses and the Will, 1888; Part II. The Development of the Intellect, 1889.

The most recent work of importance dealing with the problems of child-psychology in closest relation with those of general psychology, and at the same time doing justice to the point of view of comparative psychology, is by Bühler, while a briefer though equally commendable book by the same author brings the idea of development still more into the foreground.

Karl Bühler, *Die geistige Entwicklung des Kindes.* 4th edition, 1924.

Same author: *Abriss der geistigen Entwicklung des Kindes.* In *Wissenschaft und Bildung,* Vol. 156. 1919.

Equally modern, and filled with his own abundant experience of the subject, is the work of William Stern, *Psychologie der frühen Kindheit bis zum 6ten Lebensjahre,* 1914, 3rd edition, 1923. (English translation, *Psychology of Early Childhood,* 1924. The citations in the text are taken from the English edition.)

Among older works should be mentioned the stimulating book of Karl Groos, *Das Seelenleben des Kindes,* selected lectures, 4th edition, 1913; and the little book by R. Gaupp, which also treats of the psychology of the school-child, *Psychologie des Kindes,* in *Natur und Geisteswelt,* Vol. 213, 3rd edition, 1912.

Among works not of German origin, a book by Edouard Claparède is written from a pedagogical point of view—*Experimental Pedagogy and the Psychology of the Child* (translated from the 4th edition, by Louch

and Holman, 1911 ; a 9th edition has since appeared in the original French, 1922).

J. Sully, *Studies of Childhood*, London, 1895.

G. Compayré, *The Intellectual and Moral Development of the Child* (translated by Wilson, Part I. New York, 1896 ; Part II. *Development of the Child in Later Infancy*, New York, 1902).

These are two beautifully written older works which are stimulating, and contain much valuable material.

Finally, I wish to refer to the comprehensive work of Thorndike, which attempts to establish the principles of the science, many of which are criticized in this book. The work is not a child-psychology in the narrower meaning of the term.

E. L. Thorndike, *Educational Psychology*, 3 Vols. New York, 1913–1914.

Some monographs on the development of individual children are specified in the list at the end of the book. Reference is here made to two voluminous treatises of special subjects by William Stern and his wife, which, beginning with observations of their own children, led them to survey the whole field of investigation in child-psychology.

Clara and William Stern, *Monographien über die seelische Entwicklung des Kindes.* I. *Die Kindersprache*, 1907. II. *Erinnerung, Aussage, und Lüge in der ersten Kindheit*, 1909.

Attention is also called to four recent books by Piaget.

Jean Piaget, *Le Langage et la Pensée chez l'Enfant*, Neuchatel and Paris, 1923. (English translation, *The Language and Thought of the Child.* London and New York, 1926.)

Same author: *Le Jugement et le Raisonnement chez l'Enfant.* Neuchatel and Paris, 1924.

Same author: *La Représentation du Monde chez l'Enfant.* Paris, 1926.

Same author: *La Causalité Physique chez l'Enfant.* Paris, 1927.

The results of the two first books are summarized in the last chapter of the second (pp. 263–359), and those of the two last in the fourth (pp. 269–372).

Chapter II

GENERAL CONSIDERATIONS

§ 1—*Maturation and Learning*

We speak of development whenever an organism or any special organ becomes larger, heavier, more finely structured, or more capable of functioning. One must, however, differentiate two types of development: development as growth or maturation, and development as learning.[29] Growth and maturation are processes of development which depend upon the inherited characteristics of the individual, just as any morphological character like the form of the skull is determined at birth. To be sure, growth and maturation are not altogether independent of the individual's environment. Under-nourishment will check growth, and it may, in exceptional cases, prove permanently harmful. In the forcing-house, one can accelerate growth and blooming, but under "normal" conditions the course of these developmental phases is primarily dependent upon the laws of heredity.[30] Likewise under "normal" conditions the environment may influence growth and maturation by determining the selection of individual types of behaviour. Children who grow up out-of-doors are stimulated by their surroundings to run, to jump, and to swim, while children who are kept indoors are more likely to use their fingers than their arms and legs. The mere fact that an organ, such as a muscle, is frequently used will influence its growth quite apart from the specific character of the response; think of the many "systems" in vogue for strengthening the bodily muscles. A similar statement is applicable to

the maturation of the sense-organs. By learning, however, we understand a change in ability resulting from quite definite individual activities. In learning to play cards it is not enough that one should grow up amid favourable circumstances, or that one's fingers should have attained a certain degree of technical facility; but, first of all, it is necessary to understand the significance of a pack of cards, and of each card for itself. When some one says that So-and-so is a born card-player, he does not mean that by merely glancing at a pack of fifty-two cards spread out on a table the "born player" could sit down with three other persons and without instruction be able to play a perfect game of bridge-whist. Nor does he even mean that such a person would at once be able to play the game somehow, and would quickly master its intricacies by himself as, for instance, birds are able to fly as soon as they try to do so, and quickly attain the highest degree of perfection in this art. An ability to play cards is not thus laid down in the individual's inherited disposition. It need not develop at all in the whole course of a lifetime, and when it does develop it is a new acquisition.

In any discussion of development we are confronted with this opposition of inherited and acquired traits. Whether this opposition can be bridged over, whether that which is inherited must first have been acquired by our ancestors in the course of racial development,[31] are questions we shall here leave out of consideration. Yet this opposition is found in the development of every individual; a fact which we can only note in passing without further explanation; since to explain it would require a detailed analysis of what learning actually is, and that is one main problem of our entire book.

Nevertheless, we should have this problem clearly in mind at the beginning of our inquiry, because in their development capacities are controlled by laws inherent in the organism, and are very loosely dependent upon

the individual's achievements, whereas the abilities of an individual are chiefly determined by his experiences and achievements.

This double aspect of development makes difficult the solution of a problem to which reference was made at the beginning of the first chapter—the problem, namely, as to what part of any performance is inherited, and what part of it is acquired. In general, it has been thought possible to proceed as though whatever took place at birth, or upon the first appearance of a certain type of behaviour, could be differentiated from later forms of the same act—the former as being inherited, and the latter as being acquired. But even if it were so, this differentiation is extraordinarily difficult. Furthermore, one must not regard every improvement in a performance as an acquisition of learning; neither are all complicated performances necessarily acquired or learned; for we must not neglect the part played by mere maturation in the refinement of behaviour, both in its motor and also in its sensory aspects.

§ 2—*The Function of Infancy*

A comparative study of behaviour leads us to conclude that the higher an organism stands in the animal-series, the more helpless it is at birth, and the longer will its period of "infancy" last. The human being constitutes the extreme in both respects; his almost complete dependency at birth being associated with an extraordinarily long infancy and youth, a period which, indeed, exceeds the whole lifetime of many mammals. At no time during the entire course of his maturation does the human being attain complete efficiency, whereas efficiency is attained very early by other animals, especially by organisms much farther down the scale—which in this respect are superior to man. Infancy must therefore have a peculiar and a specific function, closely related to the superiority of the

higher forms of life. For this reason Claparède raises the question, "What is the function of childhood?" The superficial facts of comparative biology show us in what region the answer to this question must lie, since infancy is the period of greatest potentiality for development. During this period man changes from a very helpless creature into the best-equipped of all the species. In comparison, a chick can perform many acts correctly as soon as it breaks from the shell, and a full-grown hen can not do much more than a chick.

The development that takes place during infancy is also subject to conditions specifically different from those of embryonic development. The embryo's surroundings are constant, and its development is guided chiefly by a kind of immanent law, external conditions playing only the part usual in processes of growth and maturation. But all this is changed in the post-embryonic period, for the older a child becomes, the more specific is the influence which the world exercises upon his life. From this fact alone one may conclude that development becomes more and more a matter of "acquisitions"—in the sense of learning—and also, that certain stages of development are attained only after learning has been added to growth and maturation. Childhood is the period of learning *par excellence* which Claparède speaks of as the constructive period of life. Indeed, the efficiency that distinguishes the most highly-developed from all lower forms of life can not be attained simply through the intrinsic laws of development in growth and maturation. Learning is also essential to them; for efficiency depends upon functions that are not fixed in advance. When we reflect that learning, objectively considered, is an actual performance, we are better able to understand infancy, since both the extent and the intensity of the learning that goes on at this time far exceed the amount of learning in all the later epochs of an individual's life-history.

§ 3—*Parallels in Developmental History*

A comparative method of treatment has gone still further in bringing ontogenetic and phylogenetic development—or, in other words, the development of the individual and of the race—into relation with each other. Many analogies have been drawn, of varying theoretical significance, in the explanation of which many different hypotheses have been constructed. Let me introduce this topic with a statement by William Stern concerning the development of a child.[32] " The human individual in the first month of his life is a ' suckling ' whose lower senses preponderate. He enjoys but a dull instinctive and reflexive existence on the mammalian level. In the second half-year, however, the infant has attained a stage of development like that of the highest mammals—the apes [33]—furnished as he now is with the capacity of grasping, and also with a versatility in imitation. But he does not become a human being until his second year when he has acquired an upright posture and the ability to speak. During the next five years of play- and dream-life he is at the level of primitive peoples. Then follows entrance into school, and a closer articulation with the social group, together with the imposition of definite obligations, involving a sharp distinction between work and leisure—all of which constitutes an ontogenetic parallel to the introduction of man into a civilized state with its political and economic organization. In the first years of school-age the simple situations of antiquity and of the Old Testament are most adequate to the youthful mind. The middle years bring with them the enthusiastic features of Christian civilization, while at puberty he attains for the first time the mental differentiation which corresponds to present-day civilization. The period of puberty has, indeed, often been designated as the ' Age of Enlightenment ' for the individual."

I have reproduced this long quotation, not because I

believe that all the analogies indicated are truly factual, but rather to make clear the purport of Stern's view. We find here epochs of childhood compared with stages in the developmental series of animals, both the lower and the higher mammals, and compared also with human epochs, stages of culture, primitive, antique, Christian, and modern. G. Stanley Hall, who for a generation insisted upon the importance of these analogies, and who devoted both time and effort in working them out, went even further than Stern; for he found traits amongst children which recall the aquatic ancestors of man, as, for instance, paddling movements and the rapture with which the infant beholds a body of water.

It should be expressly stated that these analogies have not been taken as mere similarities, but as actual and material grounds of connection which can be employed in the explanation of development. Accordingly we shall now turn our attention to these theories; for without a doubt such analogies do exist. Typically infantile modes of behaviour, such as play, are obvious in other mammals. There are stages of child-development in which intelligent performances gradually become possible which, according to Köhler's investigation, are also typical of chimpanzees. Furthermore, the categories employed by the child in his apprehension of the world about him are quite similar to those of so-called primitive peoples. Yet these analogies are not at all limited to the age of childhood. Many adult forms of behaviour, especially when the inhibitions of education, custom, and convention fall away, are remarkably like the behaviour of apes. I may refer here to Köhler's description of the function of adornment among chimpanzees.[34] The question is, what conclusion may be drawn from these analogies? And before we proceed to an answer, our material must first be tested in a strictly critical way. In the use of analogies scientific rigor is all too readily replaced by fantastic

excursions into the realm of fiction. It is easy enough to find analogies when one is looking for them, but to separate out of the abundance of material that which, properly speaking, is alone essential to the act, is a problem that has not always been rightly solved in this field of study.

1. The Theory of Recapitulation regards the development of the individual as an abbreviated and a more or less distorted replica of the development of the race. The theory assumes that every individual passes through all the stages of development through which his species has previously passed. This is taken to be an immanent law of development based upon inherited dispositions. One thinks at once of Haeckel's biogenetic law, which states of morphological embryonic development that ontogenesis is an abbreviated repetition of biogenesis. The connection of this law with the theory of recapitulation is strongly emphasized by its advocates. The distortion, which is apparent in ontogenesis when compared with biogenesis, is explained by the different conditions under which the two kinds of development take place. Every development is, indeed, dependent, not only upon immanent laws, but also upon external influences, and if these influences happen to vary, a difference in development must also result.

The theory has many advocates, among whom Stanley Hall and his school have taken the greatest pains to formulate it in demonstrable terms. Their method is essentially this: to analyse modes of behaviour of the most general sort, and to point out those features which can not be explained as a product of learning or individual acquisition, but which may be found nevertheless in quite similar forms at earlier stages of development. In this way Hall investigated the phenomenon of fear. As an instance, he took the unexplained *pavor nocturnus*—the fact that children often awake and cry out in the night with a terror

from which it is hard to get them back to sleep again—and referred it to an atavism. The child reverts to a long-past epoch when man slept alone in the woods, exposed to danger, and was suddenly disturbed in his sleep. An important set of facts relevant to this general problem may be found in the play of children; for in play the child is supposed to re-enact the life of his remote ancestors. With the aid of Hall's questionnaire-method, one of his students collected a large mass of material concerning children's play of the most various kinds. Plays of Indians and robbers, also constructive plays of building and digging, plays of adornment, such as tattooing and filing the nails, furnish material which Hall regarded as a complete vindication of the theory; because the influence of environment, he thought, would be quite sufficient to explain the details of these varied types of activity.[35]

2. Instead of regarding individual development as a repetition of racial development, the Theory of Utility attributes both to the same causes. All development is said to result from the operation of two principles: accidental variation, and the selection of appropriate responses. In the course of racial development certain types of response arise in accordance with these principles, and either survive or are lost again. If retained in any species, the moment for the appearance of such a trait in the ontogenesis of that species is determined as a joint effect of variation and selection, rather than by the law of recapitulation. For instance, nursing occurs very early in ontogenesis, but very late in phylogenesis. The situation is reversed with respect to the sexual instinct, which appears early in racial development, but late in the development of the individual. This theory, which is vigorously upheld by Thorndike, is based upon the general theory of development associated with the name of Darwin, although Darwin and his immediate school did not restrict themselves

within the limits imposed by the "Neo-Darwinism" which has been named after him, and which employs only the two principles of chance-variation and selection.[36]

If we examine a number of individuals of the same species, we find that no two specimens are wholly alike. Individuals of the same species differ more or less from one another in the most varied ways. The uniformity of a species therefore is only an agreement of type within certain definite limits of variation. These limits of variation are assumed by Neo-Darwinism, and are considered to function in such a way that some individuals are better equipped to meet certain external conditions, while others are better equipped to meet other conditions. In the course of development those individuals better adapted to the essential features of their surroundings are much more successful in their struggle for existence. The traits of these surviving individuals are then passed on to their descendants, while those who lack these traits gradually die out. The same principles of variation and selection are again active in the offspring, so that the race is constantly becoming better adapted to its surroundings, and must therefore continue upon its course of development.

3. The third of these theories of development, which is known as the Theory of Correspondence, maintains that ontogenesis and phylogenesis are closely related processes. Since each has to do with the development of organisms, it is highly probable that certain general characteristics of development play a dominating part both in ontogenesis and in phylogenesis. In the concrete terms of Claparède,[37] "Nature employs identical means for effecting the evolution both of the individual and of the race." One may expect, therefore, that all the beginning-stages in any course of evolution will actually be of a similar nature, and that this similarity will apply equally to primitive levels, to more pro-

gressive levels, and even to the highest levels of development.

The points of difference between these three theories may be made somewhat more precise in the following manner. According to the first theory, the inherited disposition upon which the development of the individual rests is so constituted as to include everything that was ever inherited in the preceding generations of the race. All these tendencies become actualized in a serial order which is essentially determined by the order in which they arose in the ancestral series. The individual, therefore, possesses every single possibility of reaction to its environment ever possessed by the race, and the temporal order in which these different possibilities are realized is in the main determined by the original order of their succession.

According to the second theory, the tendencies are so constituted as to include only those characteristics that have been selected because of their utility, while the serial order of their appearance is determined by the biological needs of the individual, and of the species. Consequently the individual possesses only a selection from among the various possible tendencies of the past with which to react upon its present environment, the temporal order of their realization depending altogether upon their utilization.

According to the third theory, dispositional traits are so constituted that the individual indicates the history of his development from the most primitive beginnings by typical forms of reaction to his environment which appear at every stage in his career; and these reactions correspond in a general way to the stages of racial development. There are, therefore, primitive, more highly developed, and very highly developed forms of reaction, each of a uniform type, whether they be found in ontogenesis or in phylogenesis.

If we must declare ourselves with respect to these three theories, it is at once obvious that the third theory

is far more cautious than either of the other two; its hypothesis being closer to the factual data, the way is left open to further theoretical constructions. This is a great advantage, because in general the current theories of development, and especially those of inheritance, are highly unsatisfactory. The third theory relieves us from the necessity of deciding for any one theory—a decision which at best would be arbitrary—and it thereby holds our interest in the discovery of further explanations of the facts. After investigations undertaken from this point of view have yielded concrete results, we can readily use them in the construction of further hypotheses. William Stern accepts this theory when, for instance, he speaks of "genetic parallels," [38] in a concrete investigation of speech.

The theory of recapitulation, and its exaggerations, with which the reader is already acquainted from our discussion, have been frequently attacked,[39] and most energetically in his larger work by Thorndike, who rightly points out the fragmentary data and the often contradictory inferences it employs. In its principal field, that of play, the theory has also been rejected by Stern, who agrees with it only to this extent: "All psychic development, whether in the individual child or in humanity as a whole, follows certain laws of sequence, in accordance with which primitive and more roughly hewn life-forms precede the more complicated and finely differentiated. Hence the little child's games may show in their simplicity a certain kinship with the actions of primitive races, although the content of the game may be entirely borrowed from his modern environment." [40] This admission, however, is nothing but an acknowledgment of the correspondence-theory.

The utility-theory is much too closely tied up with special hypotheses to warrant our acceptance of it, because it stands or falls with Neo-Darwinism. Consequently we can dismiss both the recapitulation- and the utility-theories, and urge instead the collection of

as many facts as possible which may prove helpful in tracing the correspondence between individual and racial development. This means that one should constantly endeavour to support, to control, and to supplement the results of one branch of developmental investigation with results obtained in another branch; as, for instance, by comparing child-psychology with folk-psychology; but one should never allow oneself to be led into the dogmatic construction of uniformities and dependencies. When material enough is at hand, one can then take up the problems of dependency which naturally arise,[41] without being in any wise hindered by theoretical presuppositions.

§ 4—*The Tempo and Rhythm of Development*

Development, or the succession of its different stages, is conditioned primarily, though not altogether (see § 5), by an inherited disposition. This statement holds true both for the organism as a whole and also for its dynamics and rhythm; because these, too, are conditioned by inherited disposition. What interests us here is the fact that disposition, and therefore development, may greatly vary in these respects. In point of fact, one is able to infer dispositional differences only on the ground that different individuals when placed in the same situation and amid the same surroundings exhibit quite different forms of development. Thus, for some individuals the rate of development is very rapid, while for others it is very slow; furthermore, some individuals show a greater regularity of development than do others. A slow rate of progress at the beginning may be followed by a period of very rapid development, and, conversely, an accelerated development may suddenly be arrested, as illustrated by infant-prodigies who fail to live up to their early promise. In general, these differences may be attributed to inherited disposition, though an environment which constantly

offers strange and unchildlike problems may also contribute to hasten a child's development and early maturation. On the other hand, an environment which offers no appropriate stimulation to activity may be a serious check to development.

What has been said about development as a whole—its tempo and its variations which appear as individual differences—holds true for the individual; because here, too, we find variations in tempo, and a developmental rhythm consisting of periods in which slight advancement is noticeable from without, alternating with other periods in which development seems to take more rapid strides. Let us at once note, however, that periods of relative quiescence are not necessarily periods of stagnation; but may only be intervals in which development has taken another form. The astonishing advancement often observed in a succeeding period would be quite impossible if the child had not accomplished a considerable amount of preliminary work during the time when he was apparently quiescent. As an analogy, one can imagine a heaping-up of a great mass of potential energy during these rest-periods, which thereafter is transformed into kinetic energy. Finally, it should be observed that the rhythm of development in a single individual is not the same in all his varied functions. There are periods in which one functional complex is engaged in a particularly active state of development, while the rest are comparatively quiescent. Indeed, one might be able to characterize whole periods of life, with reference to the preferment of certain achievements, if only we were in possession of more extensive and more definite data than the present status of investigation affords. We must note, too, that developmental rhythm is subject to great individual variations; from which it is evident that the time of the appearance of any particular activity may greatly vary from individual to individual. All age-data have therefore but an approximate value for purposes of generalization;

relative statements, such as before and after, being, for the present at least, of much greater interest than absolute statements regarding the exact time at which a certain type of behaviour appears.

§ 5—*Heredity and Environment*

We have had occasion to refer repeatedly to conditions other than those of inherited disposition but affecting development—namely, the conditions set by the outer world, or environment. The question now arises: How are these two sets of conditions related to each other? This question, since it involves philosophical, ethical, sociological, and pedagogical consequences, can not be answered off-hand; yet neither can we overlook the fundamental opposition of these two tendencies as they are embodied in the well-known theories of Heredity and Environment. According to the former theory, development is determined in all its important issues by an inherited predisposition; whereas, according to the latter theory, this determination comes chiefly from environment. The same opposition is found in psychology between the rival positions of Nativism and Empiricism, according to which the quality of our perceptions—and especially those of space—is taken to be either an inborn function or a product of experience.

In contrast to both these theories, Stern advances a point of view which he calls the "convergence-theory," and which plays an essential part in his philosophy of personality. "Mental development," he writes, "is not simply the gradual appearance of inborn qualities, nor a simple acceptance of and a response to outside influences, but the result of a 'convergence' between inner qualities and outer conditions of development. . . . It is never permissible to ask of any function or quality: 'Does this come from within or without?' but rather: 'How much of this comes from within, how much from without?'—for both of these influences always share

in its making, only varying in degree at different times." [42]

It is at once apparent that we can not side with either of the extreme theories of heredity or environment; for we have already agreed that learning is essentially a type of development, and learning involves the reaction of the individual to a definite situation wherein the reaction is certainly not unequivocally tied up with inherited dispositions. But before we can proceed we must inquire into the nature of learning, and it seems to me that we can not arrive even at a clear statement of the question—much less at a final decision between psychological empiricism and nativism—so long as the problems of experience itself, and of learning, have neither been solved, nor, indeed, for the most part, recognized as definite problems.

Our aim, therefore, may be characterized by the statement that we are trying to investigate the facts which underlie the formation of all theories, and for this reason we must not allow ourselves to be hindered by the acceptance of any special theory. The concept of convergence advanced by Stern merely indicates a problem which, before it is solved must first be more clearly defined; for at present we do not even know what is meant by saying that "a certain behaviour comes from within or from without."

§ 6—*Mental and Bodily Development*

Mental development naturally goes hand in hand with the development of the bodily organism. Let us, then, briefly consider the very general connection which obtains between these two aspects of development. A few anatomico-physiological observations may be useful to us in this connection. In the foregoing chapter (pp. 23 f.) we gave a very crude description and classification of the central organ of the nervous system, explaining in particular the difference between the

"old" and the "new" brain. We may now complete our sketch by going more into detail, and considering the miscroscopical structure of the nervous system. It is not our task, however, to furnish the reader with information upon this subject; for that, reference should be made to other books.[43] We shall therefore confine ourselves to the most important facts needful in laying a basis for later considerations.

We find nerves acting as mediators between the sense-organs and the brain, and likewise between the brain and the muscles. These nerves are fibres of varying and sometimes considerable length, and also of variable thickness. They are surrounded by a protecting and insulating tissue. A nerve of this kind is not a uniform structure, but consists of a great number of separate, mutually isolated, fibres which are the real means of conduction. These fibres may be strictly classified as *sensory* and *motor*, but not the whole nerves, since there are nerves containing both kinds of fibres; as, for instance, the trigeminal nerve—the fifth cranial—which occasions the skin-sensitivity of the head and also innervates the jaw musculature; or, again, the vagus nerve — the tenth cranial — which performs numerous functions involving, among others, the regulation of breathing, circulation, and digestion. Each fibre taken by itself has, however, but one function—sensory or motor; either it leads from the periphery to the centre, or from the centre to the periphery. In this way one distinguishes *centripetal* and *centrifugal* fibres. These, however, are not independent elements; for each leads to a nerve- or ganglion-cell, and these ganglion-cells exhibit great variations both of structure and size. The common feature of all is a greater or lesser number of fibrous processes; one of these processes, called the *axis-cylinder*, being the same structure we have just referred to as the nerve-fibre. At its end this neurite divides into a fine network which closely invests either the muscle-tissue or the tendrils of another

ganglion-cell. Besides the axis-cylinder, the ganglion-cell sends out still other processes, much shorter and very numerous, often forming a network of the finest ramifications. With this plexus the arborizations of the axis-cylinders of other ganglion-cells are in close connection. It has been discovered that in many respects the ganglion-cell with all its processes forms a unit, called a *neurone* by Waldeyer. So the whole nervous system can be conceived as an organization of numberless neurones knit together with one another. Whether the connection between two neurones results from a mere contact in the fibrous network, or whether the fibrils distinguishable in the microscopic structure of the fibres form a continuous connection from neurone to neurone, is a matter which, though it has been under discussion for a long time, has not yet been decided. Without prejudice to this decision, the neurone may pass with us for a unit.

We have already distinguished between centripetal and centrifugal fibres; we must now add a third sort, namely, those which connect one part of the brain with another. "The last, the *fibræ propriæ* of the cortex, are very numerous in fully-developed brains, stretching everywhere from convolution to convolution, from the nearest to the farthest, binding whole lobes together." [44] Likewise the two hemispheres are bound together through other collections of such fibres, called commissures, the largest of which, the *corpus callosum*, is easily detected in each median section of the brain.

We now come to our particular theme, the relation between physical and mental development, which we shall first discuss from a phylogenetic point of view.

a. "Whoever knows the structure of the brain in the animal series will become convinced that the appearance of new functions is always accompanied by the appearance of new parts, or by the enlargement of already existing parts, of the brain." [45] Thus Edinger formulates as a principle of investigation the results of

his long years of research. In the phylogenetic series of vertebrates, in which, as we have seen, the "old" brain gradually associates with itself a "new" brain, Edinger seeks to point out the functions which belong to the new organ, by tracing the changes in function which parallel its enlargement. In differentiating the "old" from the "new" brain, and their corresponding functions, Edinger remarks not only that the functional activity increases enormously, but also that it takes on a new and qualitative departure, in that the behaviour of the higher animals appears to become more and more "intelligent." Paralleling this change of activity, according to Edinger, morphological changes in the brain are indicated by an increase of the areas lying between and in front of the sensory centres, and also by the growth of intercortical pathways. The investigation of these parts of the fore-brain is easy, and, indeed, these parts "clearly increase in size as the animal increases in its capacity to guide its observation and activity by intelligence."[46] Man is peculiarly characterized by the development of his frontal lobes, whereas an arrested development of these lobes goes hand in hand with idiocy.

There can be no doubt that Edinger discovered a valuable heuristic principle which he has been able to use successfully. In the course of this book, however, we shall be led to a quite different conclusion as to the nature of these activities, especially as regards intelligence, but also as regards the nature of the functions performed by various parts of the brain.

b. While the "new" brain and the ne-encephalic activities increase constantly in the ascending series of vertebrate evolution, the "old" brain is at the same time losing its independence. The higher an animal stands in the series, the less can it function without the "new" brain. Although the cerebrum has often been removed from living animals, so that their behaviour without it might be studied, there is scarcely a reported

case of a human being born without a cortex which has survived the first day after birth.

A single instance is known of an infant, lacking a cerebrum, that lived, in fact, for three years and nine months. This case is reported by L. Edinger and B. Fischer, who have compared the behaviour of this child with that of one of the dogs operated upon by Rothmann, which lived also without a cerebrum for more than three years. "The dog soon learned to run and even jump a hurdle, whereas the child lay contracted and almost motionless for three years and nine months, never making any attempt to sit upright. Neither did he attempt to grasp or hold anything in his hands. Only in his face could a certain mobility be noted, when occasionally the features were painfully distorted. Both the lips and the tongue were used together in sucking and in taking nourishment from a spoon. The dog, which at the beginning had to be fed like a child, later learned to feed himself so well that it was only necessary to put the dish before his nose and he would empty it. Of the great restlessness which dominated the dog after the restraint exercised by the cortex had been removed by the operation, making him constantly run about, nothing was ever apparent in the child. Only a continual crying was observed from the second year onwards, and this could be stilled by patting him, particularly on the head.

"The acts of bodily excretion, which took place in a normal manner in the dog, were accomplished by the child without change of position; nor did he in any way indicate when his napkin was wet. With the dog, sleep alternated with waking, whereas the child seemed always to be sleeping. The dog could not taste, smell, or hear, nor could any evidence of vision be found. This was likewise the case with the child; yet both responded with optical reflexes, and at times the eyes would close in a cramp-like manner under stimulation from light. It was not possible to find a single mental

reaction in the child, or in any way to get in touch with him, so as to teach him anything; but to a certain degree the dog could be taught, and he also gave evidence of moods, fits of temper, and periods of contented quiescence." [47]

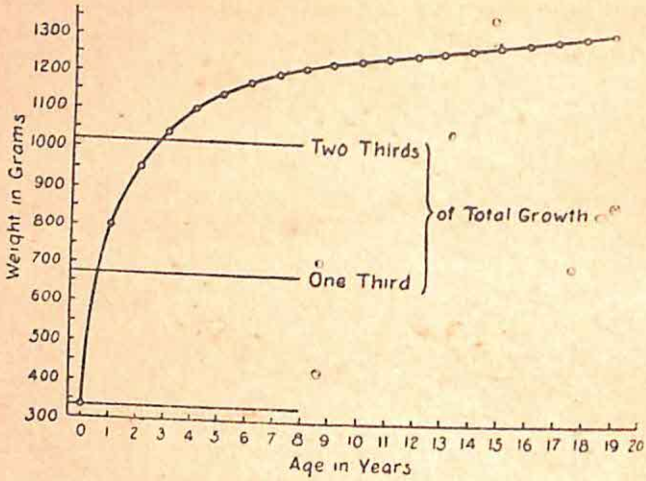
We shall return in the next chapter to the child without a brain, but the quotations already given show clearly enough how much more efficient are the same palæ-encephalic parts of the brain in dogs than they are in man, and how much man depends upon his "new" brain. We have but to compare the marked reduction in the dog's efficiency after operation with that of a fish which naturally subsists by means of the "old" brain alone, in order to have our previous thesis fully confirmed. Among all the animals man comes into the world the most helpless, and passes through the longest period of childhood. Between these facts and man's dependence upon his cortex some relationship must exist.

This leads us to *ontogenesis*. At birth the human brain is macroscopically ready; but not so in its microscopic structure. For the most part, the fibres of the brain possess no sheathing at the time of birth; therefore their function is incomplete. The maturation of the fibres goes on throughout the first months of life. At the beginning medullation takes place principally in those fibres which extend downwards from the cortex, and upon whose functioning the voluntary motion of the limbs is dependent; thence it extends to such fibres as connect the cortical areas with one another. The "new" brain of the newborn child is consequently in a very unfinished state, and on the basis of the information acquired in the last chapter, we can explain the helplessness of the child at birth by this fact, since the child, far more than the animal, is directly dependent upon the functioning of the "new" brain. Despite its unfinished state the human brain is relatively large and heavy even at birth; for the weight of the

brain is already over 300 gr., or nearly one-fourth the weight of the adult organ. In proportion to the weight of the body it is indeed heavier than in adult life, as the following figures will show :

$$\text{Child} \quad \frac{1}{6 \text{ to } 8} \quad \frac{\text{Weight of brain}}{\text{Weight of body}} \quad \frac{1}{30 \text{ to } 35} \quad \text{Adult}$$

The weight of the brain increases very rapidly, being doubled after nine months, and tripled before the end of three years; but in the course of time the rate of



[*After Bühler.*

FIG. 2.

growth decreases more and more, until the full weight has been attained at about the middle of the third decennium. (See Figure 2.)

Increase in weight parallels the development of behaviour. Weight is therefore a crude unit of measure for development, and rapid growth no doubt correlates chiefly with the first cultivation of bodily movements; although other functions also undergo their most rapid development at the beginning. A fine example of the parallel development of organ and function is found in the cerebellum, the organ which controls bodily equilibrium. That all parts of the brain do not develop in the same rhythm, and that different parts have different

epochs of particularly rapid growth, are facts or laws of mental development to which reference has already been made. Now the cerebellum grows very slowly in the first five months, then suddenly it begins to develop faster, until finally it attains its greatest rate of growth in the last half of the first and in the first half of the second year, reaching its full size towards the end of the fourth year. The time at which its greatest increase is indicated, at the end of the first year, is also the time when the child is learning to sit and to walk—activities requiring the effective regulation of bodily equilibrium which the cerebellum supplies.

CHAPTER III

THE STARTING-POINT OF DEVELOPMENT: THE NEWBORN INFANT AND PRIMITIVE MODES OF BEHAVIOUR

§ 1—*A First Survey of Behaviour. Physiological Correspondences*

BEFORE we consider development we must know its starting-point. For us the starting-point will be with the human being who has just come into the world. Embryonic development lies without the scope of our inquiry, because the mental development of a human being can not be studied until he has become an independent individual. In this chapter, then, we shall have to deal with the behaviour of the newborn child.

We must consider first of all the crude features of the infant's behaviour, and ask: What are the first actions of a human being who has just come into the world? Aside from feeding, and the vegetative functions connected with it, of which we shall soon speak in greater detail, we note a series of bodily movements, including the extension and bending of the arms and legs (these are often uncoordinated—that is, the right and left sides of the body act independently); the stretching of the limbs upon awakening; movements made in a warm bath, which movements may spread over the whole body; eye-movements of all kinds; and the most striking of all expressions—crying, whose immediate cause it is frequently impossible to determine, though usually crying can be connected with a situation in which the child finds something painful to himself; as when he is in need of nourishment, or when the environ-

ment acts directly upon his body through pressure, temperature, moisture, etc. This enumeration is by no means complete, nor is it limited to the moment of birth, but it may be considered as covering roughly the first two weeks after birth. The fact that the newborn child spends twenty hours and more each day in sleep is at least as characteristic of his kind as are any of the movements mentioned. His sleep is not one long continuous slumber, but is divided into many short periods broken by shorter periods of waking. Another general characteristic is that all movement of the limbs takes place slowly. Bühler likens this to the movements of our fingers when they are half rigid with the cold.

Both of these last-named peculiarities in the behaviour of newborn infants are elucidated by certain physiological facts. In a prolonged series of experiments, Soltmann [48] stimulated the muscles and motor nerves of newborn and adult mammals (dogs and rabbits) by artificial electrical means, and found a characteristic difference between the reactions of young and mature animals. In the newborn: (1) the irritability was much less—in general, a much stronger current being needed to produce a muscular response; (2) the form of the muscular contraction was different —in the young the contraction and release were slow instead of being sharp and sudden; (3) the onset of fatigue was found to be very rapid; (4) the muscles of the young were more highly susceptible to tetanus. When a muscle is repeatedly stimulated by intensive shocks, unless the frequency be too great, a contraction corresponds to each stimulation. But as the frequency of stimulation is gradually increased, a limit is reached at which the muscle no longer responds to separate stimuli, but remains permanently contracted in a condition of tetanus. This limit lies between 70–80 stimulations per second for the adult animal, but in the newborn it is as low as 16–18. We may without

hesitation apply these results to the human being. We can then understand the slowness of the infant's movement from Soltmann's second result; the great need of sleep from his third; and the capacity to regain sleep so readily from his first. We adults, on the contrary, find great difficulty in falling asleep during the daytime, even when very tired, because of the many stimuli constantly influencing our sense-organs. But in the case of infants, their sensitivity being less, such inhibitions are much weaker.

Furthermore, I believe an analogy can be traced between the conduct of the newborn infant and Soltmann's fourth result, though this analogy refers to the sensory and not to the motor aspect of tetanus. By stimulating sense-organs periodically one can obtain the same kind of uniformity in the phenomenal effect that is found in the tetanus resulting from recurrent muscle-stimulation. Take the most familiar and thoroughly investigated instance of this—the sense of sight. If one casts light upon the eye for definite periods of time separated by intervals of complete darkness, by means of a rotating disk, or colour-wheel, half white and half black, a slow alternation between bright and dark is observed when the rate of rotation is slow; but if the frequency is increased a new phenomenon occurs: the disk begins to flicker. A still further increase in the frequency of rotation brings us to a limit beyond which the rapidly revolving disk of black and white sectors appears like a uniform gray, completely at rest. The occurrence of this uniform impression is known as *fusion*, and fusion corresponds to tetanus. But the correspondence of these two results extends still further; for the laws upon which these effects depend—the conditions influencing the limits of tetanus and fusion—are the same.[49] Therefore the inference may be drawn that the critical frequency for fusion—that is, the lowest frequency that will just produce it, which in the case of adults is about 50 periods per second[50]—might be very

much lower for infants. This fact may be difficult to prove; but at all events nothing is now known that runs counter to such an inference.

The results of certain investigations which I conducted jointly with P. Cermak, showed that a close relationship exists between this phenomenon of fusion and the visual perception of movement. I will only indicate the fact that when a movement is made too rapidly it loses the phenomenal characteristic of seen-motion; and what we then perceive is a motionless streak of light, instead of a moving point.[51] The laws here involved are the same as those controlling fusion.

In conclusion, we may infer from Soltmann's fourth statement that in the perception of movement the limen at which movement disappears is more quickly reached (that is, at a lower speed), in the case of newborn infants than it is in adults, an inference which fits the known facts perfectly. Although authorities are at variance as to the time when a child begins to follow a moving object with his gaze, they are agreed that the child can accomplish this act only if the movement of the object *takes place slowly*.[52] Up to the present, these observations have been referred chiefly or wholly to the development of the motor side of this performance—the arousal of the eye-movements which follow the moving object, and which are supposed to result from the successive stimulation of different points on the retina. That is to say, the explanation was supposed to be furnished by a "connecting mechanism" operating between the sensory and the motor parts. But perhaps the sensory performance itself should be included in the explanation. Inasmuch as we shall soon become acquainted with a conception of this "connection" which establishes a very close relationship between the sensory and the motor aspects of the optical apparatus, I, for my part, am ready to conclude that, as a matter of fact, infants do have far less capacity than adults to *see* movements, and that this Koff

deficiency is directly related to the more ready onset of tetanus in the young.

In this connection a question arises. If our assumption regarding the defective motor-vision of newborn infants is correct, it appears that we are dealing with a performance which improves during the course of life. Shall we then conclude that experience accounts for this change? By no means, for if our other assumption, which would bring this fact into relation with the facts of muscle- and nerve-physiology, is correct, it is not "experience" which accounts for the gradual increase of the limit from 16 to 80 periods of stimulation per second at which tetanus takes place, but, evidently, a physiological alteration of the organ which, in the preceding chapter, we have called *maturation*.

The process of maturation would then be the occasion for development in the perception of movement, and there is no reason to suppose that this development can be referred to experience alone.[53] Furthermore, we have here a most instructive example of the possibility mentioned in the last chapter of interpreting development in terms of maturation. We shall meet with this problem again, when we come to speak of eye-movements.

§ 2—*Is the Newborn Infant a purely "Old-Brain" Type of Being?*

We already know that most of the connections between the "old" and "new" brain of the newborn infant are neither medullated nor conductile. In addition, Soltmann obtained the following results: Until the tenth day after birth no sort of movement of the body- or head-musculature could be aroused by electrical stimulation of the puppy's cortex, though with older animals movements were readily produced by this means. Furthermore, destruction of the motor

cortical areas, which in older animals results in a severe disturbance of movement, produced no interruption or paralysis of the muscular apparatus during these first days of life. When one considers these facts, and the points noted above with reference to human beings, one is tempted to infer that the newborn human being is also a purely palæ-encephalic creature. It has also been observed that the behaviour of children lacking a cerebrum (anencephalic children) does not appear to differ in any important respect from that of normal children. For instance, children without a cerebrum cry at birth just as normal infants do. Yet the case described by Edinger and Fischer, to which reference was made in the foregoing chapter, does not seem to agree with such a conclusion.[54] "The child accepted the breast immediately, and from the first nursed in the right way; but really, the child was awake only at the time of nursing, and before it would nurse it had to be wakened. Otherwise, it always lay as if 'in sleep.' It was never heard to cry during the first year, but only occasionally to utter a low tone." [55] From this account it appears that the behaviour of the Edinger child must have been somewhat different from that of a normal infant, even from the very first days of its life, because in normal infants a facial expression of contentment can at times be observed (Preyer), whereas Edinger's infant did not indicate the slightest facial expression during its entire life—except for an occasional "painful distortion." It therefore seems probable to me that in healthy newborn children the "new" brain already plays some part in determining their behaviour, although we can not yet tell how. Soltmann's investigations with dogs thus furnish an inconclusive parallel, because, as we have seen, the human being is dependent upon his "new" brain to a much greater extent than the dog.

One can very soon discern the growing influence of the "new" brain in the course of normal develop-

ment; which is but another instance of the process of *maturation*.

§ 3—*Impulsive Movements*

When we consider the movements of the newborn infant described in § 1, we find that few of them are correlated with definite external stimuli or with determinable situations; and hence they do not appear as reactions, but give one the impression of spontaneity. In this sense they are aimless or purposeless, inasmuch as they do not attain a recognizable end. These movements have therefore been distinguished as a group, termed by Preyer "impulsive movements." Their physiological origin is also implied by this distinction. Preyer regarded them as a continuation of embryonic movements, "which the fœtus already executes, and earlier than any others, at a time when, as it can not possibly be incited to movement by peripheral stimulus, its centripetal paths are not yet practicable, or not yet formed at all, and the ganglionic cells from which the excitations proceed are not yet developed."[56] Since, however, no movement can occur without a stimulation of the motor nerves, he concludes that internal physiological processes, such as nourishment and growth, must occasion these impulsive movements; a conclusion in which Stern agrees.[57] This view is generally accepted in so far as it states the fact that these movements, unlike the spontaneous responses of adults, are aroused neither by external stimulation nor by excitations of the cortex. In their description, however, one must add, as both Stern and Thorndike have remarked, that objectively considered, they are by no means useless. On the contrary, their function has a considerable value to the individual in promoting the growth and maturation of their respective organs.[58] Stern calls this a *pre-practice* value, while Thorndike, in accordance with his Theory of Utility, which was discussed in the last

chapter, regards this value as an explanation of the arousal and conservation of these movements in the development of the race. Thorndike proceeds, then, to argue against a sharp distinction between this group of movements and any other; and, indeed, it is true that impulsive movements ought not to be regarded as if they were entirely independent of the situation, or purely arbitrary in their nature. If one could fully understand the total situation, which in instances like these mainly involves the conditions and processes of the nervous system, one would find that all impulsive movements are strictly regulated. This, of course, needs to be emphasized, but a certain distinction still remains, inasmuch as the impulsive movements are specifically attributable to inner situations, whereas other movements are expressly conditioned by external situations. Yet even when so considered the distinction is not very important; for it matters little whether a child cries because it needs food or because its leg is being pinched. We shall pass on, therefore, to a consideration of the more significant behaviour which occurs in response to definite external stimuli, adding that in the course of development these so-called impulsive movements retreat more and more into the background.

§ 4.—*The Reflex-System*

In a second group we may place a type of behaviour which occurs in response to external stimuli. These movements have a number of peculiarities: (1) The reactions as well as the stimuli are relatively simple. This is not an exact description, because it is not easy to define what is meant by "relatively simple." But the statement will serve to distinguish these movements from a third group yet to be considered. (2) The movements of this second group take place with extraordinary uniformity. That is, the situation remaining

the same, identical stimuli always produce the same reaction, unless, indeed, the irritability of the organism deviates from its normal level, toward hypersensitivity on the one hand, or toward fatigue on the other. (3) Variation of the stimulus in a certain direction, such as a gradual increase of intensity, does not always produce an alteration of the reaction in the same direction; for the reaction may suddenly become qualitatively different, sometimes because an organ hitherto quiescent has been called into action. (4) These movements depend upon the inherited make-up of the individual, and do not have to be learned. (5) They are of the greatest utility to the organism, consisting, in general, of protective, defensive, or adjustive movements, as is obvious from any description of their separate types. (6) Still another uniformity may be mentioned. The reaction can be changed when, in addition to the normal stimulus, another stimulus is applied at some other point.[59] We call these movements reflexive, or, briefly, *reflexes*, and an example would be the contraction of the pupil when the eye is stimulated by light.

Before entering upon a discussion of the reflexes of the newborn infant, let us glance at some of the ideas which have been advanced in their explanation. We might ask the question: How must an organ be constructed whose function is destined to be reflexive? The usual answer to this question is very simple. We know two kinds of nerves, anatomically and physiologically—namely, sensory and motor nerves. Furthermore, we know that sensory nerves possess a terminal arborization which, either directly or through the mediation of other neurones, approaches the terminals of the motor nerves; and we know, finally, that an injury at any point of this more or less complicated series of neurones involved in the arousal of a movement interferes with the movement itself. The function of the reflex also indicates the double nature of the stimulation and

response. The organ of the reflexes is therefore quite obviously a more or less complicated chain of neurones which, in the limiting case, may consist of but two members. Always beginning with a sensory neurone and ending with a motor neurone, this apparatus is called a *reflex-arc*. One should not overlook the fact, however, that these reflex-arcs are not isolated mechanisms, but are connected with other parts of the nervous system, as can be demonstrated both by the facts concerning facilitation and inhibition already mentioned, and also by the fact that many reflexes can be voluntarily influenced; as, for instance, sneezing can be repressed voluntarily for a longer or shorter time.

Although investigators, perhaps, have not always been conscious of it, current theories of reflex-action have shaped their views concerning the reflex-organ in a very definite way. It has been customary to consider the reflex-arc as composed of a centripetal and a centrifugal branch, these being regarded as independent parts, while the characteristic feature of the apparatus was the *connection* that exists between them. A reflex-mechanism is then conceived as a *pre-determined, inherited connection* between afferent (receptor) and efferent (effector) pathways. Such a formulation of the original data is, of course, readily inferred. Anatomically the parts can be separated, and in accordance with the principles of the assumption one can easily imagine a mechanical scheme of explanation. Such a scheme also satisfies our reasoning to a considerable extent, because it is readily comprehended, and thus seems to be a good explanation.

But before we accept this hypothesis we should look more closely into the functional aspects of the mechanism involved. What happens in the reflex-arc when a reflex movement is made? Obviously the energy arising from the external stimulus can not be simply transformed into a nervous process. Such an assumption would be untenable for any kind of nervous action.

The effect—the movement of reaction—stands in altogether too loose a relation to the energy of the stimulus to warrant such an assumption. The only possibility is that the stimulus activates energy which lies stored up in the nerve-cells. At the same time the stimulus may co-operate very materially in determining how much and what kind of energy shall be released; but the only energy available is the energy already present in the nerve-cells.[60] This conclusion holds true for the motor as well as for the sensory nerves. If I stimulate a motor nerve directly by electricity, it is not the electric impulse itself which is conducted to the muscle, causing it to contract, for here again we have only a release of energy. Assuming, then, the independence of the centripetal and the centrifugal neurones, the reflex takes place as follows: The stimulus activates a certain amount of energy in the sensory neurone, which, passing along the neurone, in its turn activates the energy stored up in the motor neurones; the relation between the processes in the sensory and motor neurones being of the same order as that between the stimulus and the sensory process. At any rate, the stimulus can have nothing to do with the movement of reaction. While such an apparatus may be called a *mechanism*, the teleological character of reflex-movements is not accounted for until still further assumptions are made, which can be better understood after we have discussed a third group of movements.

To complete the picture of the reflex, we should add that reactions may in their turn stimulate sensory nerves, thus apprising the nervous system that a movement has been made, and what kind of a movement it was. This does not mean that we ourselves must become aware of it; for many reflexes take place altogether without consciousness, just as other movements do when their reflex-arcs have been cut off from the "new" brain. An instance of this was described in the first chapter—the case of the woman who gave birth without being aware of it.

We have emphasized the strong points in the theory of the reflex-arc, and have also called attention to a deficiency in it. Further defects will become evident as we turn now to consider the reflexes evinced by newborn infants.

§ 5—*The Reflexes of Newborn Infants. The Problems of Eye-Movements*

From the very outset, all manner of reflexes take place upon stimulating any of the infant's sense-organs. These reflexes have already been subjected to thorough investigation over a long period of time. We shall here limit ourselves to a few examples.

(a) *Eye-Reflexes.*—The pupillary reflex is bilateral from the very first; that is, when light is directed into one eye only, both pupils contract. The lids of the eyes also function from the beginning by closing whenever the eyes are stimulated with light; at first, however, they do not close when an object approaches the eye rapidly. A much disputed problem is that of the eye-movements which adapt the eyes in their position and adjustment with reference to the outer world, so as to provide the individual at all times with the most effective use of his organs of sight. In us adults these movements occur automatically, like reflexes, and they are co-ordinated; in the newborn infant, however, they are sometimes entirely unco-ordinated. Indeed, the infant can readily move one eye, while keeping the other one perfectly still. For the present it is well to separate the two problems here involved; first, the problem of the direction of the eyes toward a certain object, or *fixation*; and secondly, the problem of co-operation, or the *co-ordination* of the two eyes. In fixation, the eye is turned until the fixated object falls upon the place of clearest vision lying at the centre of the retina (the *fovea centralis*), while the lens assumes a degree of curvature such that a distinct image of the object is focused

upon the retina (*accommodation*). Co-ordination, on the other hand, consists in keeping the accommodation and fixation always the same in both eyes (this is called *convergence*).[61]

Do eye-movements, then, belong among the inherited reflexes, or are they acquired? First let us consider co-ordination; two diametrically opposed theories have been here advanced. According to Hering, "the co-ordination of movements in the two eyes depends upon an inborn arrangement, and not upon exercise. So far as concerns their movements in the service of vision, both eyes may be taken together as constituting a single organ."[62] It is not as though each eye moved by itself, because a single impulse suffices to occasion a reaction in both eyes, just as if the organs were a double-eye.

On the other hand, Helmholtz observes "that although the necessity of moving both eyes together . . . appears to be something which can not be overcome in normal vision . . . it can be shown, however, that the regularity of this connection is a result of practice."[63]

We have before us two opposed theories which have dominated the whole psychology of space-perception. From the one point of view the essential feature of behaviour—which is, in our case, eye-movements—is explicable on the basis of pre-determined, inherited dispositions. Individual life, practice, experience, all serve in the perfection, but introduce no new forms of behaviour. In accordance with the other theory, however, the essential features of behaviour are conceived to be a result of practice. The first theory is called *nativism* and the second, *empiricism*.

Of the various arguments that have been advanced on both sides, we shall consider in the main only those that have a bearing upon our particular theme—the psychology of infancy. The chief argument of Helmholtz lies in the fact that one can learn in some measure to destroy the co-ordination of the two eyes. The

inference is then drawn that what can be altered by practice, must also have been acquired through practice. This argument, however, is not at all convincing; for it is unnecessary to suppose that an inherited co-ordination must involve an insurmountable compulsion towards behaviour. It is easy enough to demonstrate that other inherited modes of response are modifiable through practice. Hering, for instance, notes that one can train a four-footed animal to adopt a pace unnatural to its kind, as does the trotting horse.

The empiricist in his turn might seek to support his views by reference to the unco-ordinated eye-movements of the infant, were it not for the fact that co-ordination has been observed even in the first day after birth—the movements in some children being altogether co-ordinate.[64] This is a true co-ordination and not the mere simultaneous response of each of the two eyes to the same stimulus, because one can screen one of the infant's eyes without interfering at all with its co-ordinated eye-movements.[65] This fact becomes even more convincing as a support of nativism, inasmuch as the newborn infant often moves both hands or both legs at once, and when these movements are co-ordinated at all they always take place symmetrically—that is, in opposite directions; never in the same direction. The hands, for instance, are moved towards one another or apart from one another, but never simultaneously to the right or to the left. Indeed, Hering calls attention to the fact that it is not easy even for an adult to move the hands quickly to and fro at the same time in the same direction.[66] Let the reader try this experiment for himself and he will be astonished to find how difficult it is. On the other hand, even infants can move their eyes in the same direction with the greatest ease at every turn of the gaze from right to left, or from left to right. Consequently the co-ordination of the eyes can not be altogether a result of practice, but must have its foundation in an inherited disposition. In

support of this conclusion it may be added that unco-ordinated eye-movements usually occur under conditions favourable to impulsive movements, such as those observed when the child is placed in a warm bath. Likewise in older children, unco-ordinated eye-movements have been observed when they are asleep. Furthermore, an experiment with animals, involving direct stimulation of the *corpora quadrigemina*, a nucleus of the "old" brain, always results in co-ordinated eye-movements.

From this last fact, the inference may be drawn that co-ordinated eye-movements are called forth by the central organ of the brain, and that atypical, non-co-ordinated movements have a quite different origin, and have nothing specifically to do with vision.[67] If we recall what has already been said regarding impulsive movements, we shall find a warrant for adding the un-coordinated eye-movements to this group of responses.

Our conclusion is that an extreme empiricism certainly can not be maintained, since the inherited structure of the central nervous system must play a part in the co-ordinated movements of the eyes. So far, at least, agreement may be said to have been reached by all investigators.[68]

Let us turn, then, to a consideration of the problem of fixation. Here, as recent investigation shows, the situation is similar. Although the suckling's eyes wander for the most part irregularly, under favourable conditions fixation may occur even during the first day after birth. This fact has been demonstrated by Watson,[69] who experimented upon twenty newborn infants. The child was placed on its back in a darkened room, with its head held in a constant position. A point of light was then shown, and in many instances the infant's gaze followed the light when this was moved within a radius of 20 degrees from the centre. Watson naturally chose for his experiments conditions which are most favourable for fixation. Concerning

these conditions Exner has shown that an adult can fixate a light indefinitely and without difficulty in a darkened room; whereas under other conditions fixation is difficult, painful, and at length becomes impossible.

It appears that under normal conditions moving objects are the first to attract the infant's gaze. Stern [70] and Guillaume [71] both report of their children that an object like a watch or the father's finger will be followed accurately with the eyes on the fifth day after birth.

Another early achievement is the cessation of irregular wandering of the gaze as soon as the eyes catch sight of a lighted object. The child then *stares* at the object. This behaviour occupies him profoundly. Under certain circumstances the infant can even be induced in this way to stop crying. Whether "passive" fixation—so-called because it is produced by an interruption of movement—occurs in normal surroundings at an earlier date than "active" fixation, can not be stated with certainty from the observations of Stern and Guillaume. Furthermore, we do not know whether a moving object at the point of regard is at first a stronger stimulus for fixation than one exposed at the periphery; although general considerations, as well as certain observations made at very early dates by Stern and Guillaume, make such a conclusion probable. In any event, I can not agree with Bühler [72] that these two experiments amount to the same thing; for in the first one the stimulus is a stationary object lying at one side; while in the second it is a moving object in the centre of vision. Although it may be quite true that even adults follow a movement with the gaze by fits and starts, so that the eye is always following small displacements of the object from the middle toward one side or the other, still a moving object is never the same as an object at rest. The conditions of movement are therefore different in the two cases.

From the beginning, therefore, an active fixation is possible, although at first it may not be perfect. For instance, the eye-movements occasionally overshoot the mark, or fall short of it.

The development of these functions takes place with reference to the conditions under which fixation occurs. A pure empiricism is therefore impossible, and a difference of opinion can obtain only with respect to the completeness of functioning. While the empiricist regards the observed development as a process of learning, the nativist regards it as a process of maturation.

Investigators now tend to accept both factors, and to admit that inheritance and acquisition are alike involved in fixation and in the co-ordination of the eyes, without attempting to limit the participation of either.[73]

But what does it mean, to say that the movements of fixation result from an inborn pattern, or, in other words, that they are true reflexes? The behaviour consists in turning the eye so that a stimulus anywhere in the field of vision will be brought to the centre of the retina; or, stated differently, an image of a luminous point anywhere on the periphery of the retina acts as a stimulus for movement which brings the point to a focus upon the fovea. "When carefully examined, these processes reveal a complicated and finely differentiated system of interconnections between the impressions of light upon separate points of the retina and the specialized impulses of eye-movements. Strictly speaking, a different movement must arise from every retinal point; *therefore, every fibre of the optical nerve must have a different central connection with the motor nerves which innervate eye-movements.*"[74] This statement of Bühler agrees entirely with what we have already learned about the reflex-apparatus, but the conditions must actually be much more complicated, as the following consideration will show. Assume that the gaze of a child is first of all directed straight ahead

upon a point A (see Fig. 3). There appears now in the same plane a point of light at B on the right. The eyes will then move so that this point falls upon the fovea. If now another point of light B_1 is introduced vertically above B, the eyes will move upward and fixate it. Let us assume that the eyes are again directed upon A, after which a point A_1 is flashed vertically above it. In passing from A to A_1 the same retinal position will be affected which received B_1 when the gaze was first directed upon B. Again there is an upward movement of the eyes to effect the fixation of A_1: but although in this case A_1 stimulates the same retinal point which in the case of the first retinal movement from B to B_1 was stimulated by the point B_1, yet the two movements are not at all the same, because the movement from A to A_1 and that from B to B_1 require different innervations of the eye-muscles. What is shown in this special case may be stated in general terms as follows: the innervations which the eye-muscles undergo in movements of fixation are determined, not only by the position of the retinal points which arouse the movement, but also by the pre-existing position of the eyes. It therefore follows that every sensory fibre must possess not merely one connection with the motor nerves, but as many as may be required for all possible positions of the eyes. This means an enormous multiplicity of connections, among which those that function in a special instance must always be determined by the position of the eyes. It would be a hopeless task for the biologist to explain a structure, the marvellous complexity of which would assure an appropriate function, without the service of this function in guiding its development.

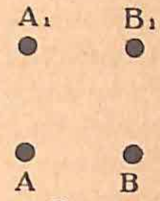


Fig 3.

Referring again to our example, it appears that the movements from A to A_1 and from B to B_1 are actually different. The optical, centripetal impulses which re-

lease the two pass through different connections, and yet each time the movement leads to the same end. In other words, as a result of both movements a point placed above the original point of fixation becomes itself the fixation-point. An internal connection between the same end and the different means whereby it is attained can not exist in the way the general theory of reflex-action has provided; for although the reaction is in each case linked with its stimulus, the sensory and motor processes involved are quite heterogeneous. Accordingly, the question arises in our minds whether any such system of connections can be assumed to be at all probable. And this question persists and, indeed, becomes more insistent when we attempt to explain the movements of fixation in purely empiristic terms; for, as we shall see, according to current teaching learning is nothing more than the establishment of these specific connections between neurones. The difference between Nativism and Empiricism does not refer to the part played by a fixed system of connections in eye-movements, but only to the establishment of such a system. Empiricism recognizes the difficulty of explaining how, prior to any functional activity, so complicated a system can have arisen, capable of assuring such definite and graded achievements. The true situation is revealed from another side when we realize that every optical nerve contains 1,000,000 fibres—while no more than 634,000 fibres issue from each half of the spinal cord—and that three pairs of nerves, the third, fourth, and sixth cranial, regulate the movements of the eyes by their manifold combinations.

As for experience alone, it does not furnish an explanation either—because, as we have seen, fixation is possible at the outset, and, as we shall see, an "empiristic" concept of experience is itself untenable. Consequently, the solution must lie beyond the concepts employed in the empiristic-nativistic controversy. We have simply come to a point where modern psy-

chology must relinquish some of its older views, and accept instead certain new principles to which we shall have occasion to recur again and again in this book. Among other things, these principles will also be found to have a significant bearing upon the theory of learning.

Going back to the older theory of eye-movements, we find in the optical sensorium and motorium (I believe these terms will be readily comprehended) two distinct types of apparatus which are bound together simply by a multiplicity of connections. Consequently sensory and motor processes in the optical field will have as much or as little to do with one another as would be the case with any other reflexes. This is the view which dominates to-day, yet it runs counter to the fact that eye-movements are determined to a very considerable extent by the characteristics of the visual phenomena which are involved. As a proof of this fact, the reflexes of fixation which we have described are merely one example. To mention another, eye-movements are dependent upon the contours of seen-objects. By means of accommodation, the fixation of a contour affords a sharp image upon the retina. Furthermore, eye-movements are so co-ordinated that, apart from a few unimportant deviations, every position of the two eyes provides for the reflection upon corresponding points of the retina of the largest possible number of external points furnished by the stimulating object [75]; and that, whatever position the eyes take, a horizontal line passing through the fixation-point will always fall upon the same lines on each retina.[76] Briefly stated, the principles according to which our eye-movements are regulated are so determined that our visual perception furnishes the clearest possible purview of surrounding space.

The "beautiful harmony" between the sensory and the motor functions of binocular vision has already been appropriately emphasized by Hering. But so long as all the functions involved were considered to be held

together as a mere connection of individual elements, it was impossible to comprehend the significance of this harmony. Is there, then, no other possible conception whereby this harmony can be understood? According to Wolfgang Köhler's fundamental work [77] it appears that there is. But Köhler's conception is of quite a different order from Hering's, and agrees instead with certain ideas which Wertheimer has recently introduced into psychology. What these ideas are will be made clear in later chapters; here we must confine ourselves to the new explanation of eye-movements which they afford. First of all, the assumption is definitely renounced that the relation between sensory and motor functions in optics is a mere system of interconnections; and with this renunciation go all the consequences of the previous assumptions which we have pointed out on pp. 78 f. For instance, we can no longer assume that the sensory function serves merely to release the motor function without involving any *inner* or *material* connection between the two. Instead, the hypothesis is advanced that the specific pattern of the seen-object itself regulates the movements of the eye. From this it follows at once that the optical sensorium and motorium can not be regarded as two independent pieces of apparatus, since for many types of performance they constitute a *unitary organ*—a physical system—within which separate organic parts may react upon other parts. Accordingly, what happens at one point in the organism is never independent of, or without its influence upon, what is taking place at any other point in the organism. The significance of this new conception of psychology can be revealed only gradually in the course of this book.

Thus we have an entirely new explanation of eye-movements, according to which our optical organ, sensory plus motor, becomes a self-regulating apparatus. By operating upon the motor parts, the sensory event alters its own conditions. What happens is shown in

Watson's experiments on fixation. When the infant turns its eyes towards a single point of light exposed in darkness, the movement is occasioned by processes in the optical sensorium. This movement shifts the image of the light on the retina and thereby changes the process in the sensorium. "This action, however, is a complete circuit. Every influence upon the position of the eyes immediately affects the conditions, so that the question arises whether in such a process reacting upon itself anything arbitrary or irregular can possibly take place" (Köhler). The answer is that the eyes can only move in such a way that an improved state of equilibrium is established in the field of perception.

The *fovea centralis* (the place on the retina where the fixated point of light falls) is a functionally outstanding position, because it is the "centre" and the most efficient region. The equilibrium of the sensorium will therefore be better when a single point of light coming from without attacks this region than when it attacks any other. In less simple cases the conditions are not so readily described, but it will always hold true that changes take place in accordance with the greatest possible simplicity and equilibration of forces; and the principle of the greatest horopter (cf. p. 81 and note 75) will be found in harmony with this requirement.

To go into the matter on its physical side would take us too far afield; but the main point to be noted is this, that a connection between two different functions is possible without the provision of a special mechanism to account for it (cf. pp. 70–72).[78] I repeat that the reader can not be expected at once to fully comprehend the significance and the importance of this new principle. But when the same ideas have recurred again and again in connection with different problems, this end will have been attained, and the reader can then turn back to these pages and review this section.

One concluding observation is here in place. Eye-movements may still be termed reflexes, although, as

we have seen, they can be explained without the assumption of any special mechanism conceived as a system of mere interconnections, which leads us to question whether we can not apply this explanation of eye-movements to all reflexes. We shall only raise the question at this point, but will attempt something in the way of a answer to it in § 9.

So much, at least, is clear: The question whether the empiristic or the nativistic theory of eye-movements is right—whether these movements take place according to inherited laws, or whether they must each be learned by individual experience—now assumes an entirely different meaning. Since the visual phenomena themselves, or at least their physical correlates, regulate eye-movements by virtue of their specific qualities, it follows that in the course of development eye-movements must depend upon the phenomena which go with them. Progress in any performance, such as visual fixation which we have been discussing, will therefore be partly conditioned by the progress made in the act of seeing itself. Here again Empiricism and Nativism are as bitterly opposed as ever, but a decision between them can be reached only after we know what "inherited" means, and after we have taken up the problem of learning.

Returning now to the list of the reflexes found in newborn infants, a few more examples may be added.

(b) *Ear-Reflexes*.—In the beginning specific reactions to auditory stimuli are lacking (cf. pp. 134 f.), but during the third or fourth month—according to Guillaume, even in the first days—a response is developed that appears to be like the eye-movements of fixation, when the infant turns his head in the direction of a sound. In Preyer's son this reaction had attained "the regularity of a reflex-movement" in the sixteenth week. According to Miss Shinn's observations, turning the head towards a sound at the right or left is executed much more promptly and accurately than towards a

sound located above or below ; the latter adjustment being made with considerable difficulty by her niece even at the end of the second year. The sound-reactions of infants have been systematically investigated by Hetzer and Tudor-Hart, also by Löwenfeld. In both studies the reaction of turning the head toward the source of sound was observed, and in one of them the reaction took place within the first three days after birth. We now know that the localization of a sound to the right or left depends upon the time-sequence in which the sound-waves issuing from [illegible] strike the right and left ear, respectively. Since a sound coming from the median plane between the two ears strikes them simultaneously the act of turning the head has the effect of bringing about this condition, and the condition appears to be a simplification of the excitatory processes in the brain-centres where the separate excitations of the two auditory nerves are united. Again, as in the case of eye-movements, the system alters its own conditions in the direction of maximal simplicity. The advantages of this hypothesis are obvious, especially in view of the difficulty involved in constructing a satisfactory hypothesis in terms of bonds of connection ; for what would the bond connect ? An impulse to move with a difference in time ? According to our hypothesis the amount of the difference in time determines the magnitude of a movement requisite to abolish the difference, and to permit the two ears to be simultaneously excited.

The greater effectiveness of right-and-left over up-and-down localization lends support to this interpretation ; so also does another observation made by Miss Shinn that *continuous* sounds, such as playing on the piano (forty-fifth day), were the first auditory stimuli to cause the turning of her niece's head, whereas brief sounds like sneezing did not occasion this reaction until the ninety-second day. Löwenfeld's systematic observations confirm this result.

Miss Shinn not only records the turning of the head, as Preyer does, but also the direction of the child's gaze, and Löwenfeld has discovered that the turning of the head precedes the direction of the gaze. Further investigation is needed to throw light upon the connection between these two reactions.[79]

(c) *Skin-Reflexes.*—A considerable number of reflexes are aroused by stimulation of the skin. Among these, one that is typical of the newborn infant is the so-called Babinski-reflex, which after a few weeks is supplanted by the plantar reflex, and does not again appear in the normal adult. If one touches the sole of a newborn infant's foot, the toes are stretched upwards and outwards. This is the Babinski-reflex. If the same stimulus is applied later in life it causes the toes to move downwards and press together, which is the plantar reflex.

The Babinski-reflex appears to have a protective, or flight, character. A similar reflex can be released in infants by touching the eyelids or lashes, which is immediately followed by closing the lids. In the sense of a positive adaptation, still another reflex is effective, even in the case of an infant without a cortex. If one touches the palm of an infant's hand, the fingers close about the object with which the hand has come in contact. In this connection one should also mention the remarkable reaction called the "clinging" or grasping reflex. In the hand-closing reflex the child exercises an extraordinary force. In America, Robinson has made a special study of this reaction, and has found that a great many infants, not yet an hour old, will grasp a small stick so tightly with their fingers that one can raise them in the air. Twelve newly-born infants hung thus for half a minute, like gymnasts on a horizontal bar, and three or four held on for fully a minute.[80] As an addendum to these remarks, it may be noted that the vegetative processes take their normal course from the beginning, although breathing

and the pulse-beat are much quicker and less regular in infants than in adults. Reflexes, such as sneezing and coughing have also been observed during the first days after birth.

§ 6—*The Suckling Instinct, and the Primary Characteristics of Instinctive Movement*

We shall pass over further details concerning the reflexes, and turn to a third group of movements. Up to the present we have not touched upon one of the most frequent, most important, and most characteristic of the infant's forms of behaviour : its mode of nourishment by suckling. Immediately after birth the child is able to suckle and swallow the milk. When the nipple is placed between its lips this characteristic behaviour either begins at once, or within a few minutes, during which less appropriate movements are being made. Suckling is not so simple a reaction as it might at first seem ; for it requires the exact co-operation of the muscles involved. The lips must surround the nipple so as to exclude air, and the movements of sucking must take place with a rhythm of the contracting and expanding muscles which is in time with the movements of swallowing ; and yet " of all the movements of the 'suckling,' hardly any is so perfect from the beginning as that which gave him his name." [81]

The suckling movement is not continued indefinitely, nor until fatigue sets in ; for when the infant has taken a sufficient amount of nourishment it refuses the breast, and will no longer suck even if one places the nipple again in its mouth. When, on the other hand, a child is hungry or in want of food, suckling is induced not alone by the nipple, for the infant will also suck a finger or the cheeks of its mother whenever its lips come in contact with them ; showing that it is not necessary to introduce milk into the mouth in order to stimulate the reaction. Not that any object placed in

the mouth will necessarily be sucked; for, as Preyer has pointed out, the object must not be too large or too small, too hot or too cold, too bitter or too salty. It is likewise important that the milk should be of a proper consistency, otherwise the act of suckling is interrupted. Thus Preyer reports that on the fourth day his child refused cow's milk thinned with water, which on the second day he had taken without hesitation, and not until a small amount of sugar had been added could he be induced to receive the nourishment. This behaviour of suckling is likewise evinced in infants without a cortex. The child described by Edinger and Fischer also "took the breast at once, and suckled properly from the beginning." A certain difference between normal children and idiots, especially those lacking a cortex, seems to be indicated by the fact that normal children perfect the act in so short a time that, as Preyer reports, it takes place with machine-like regularity after about two weeks. According to the observations of Sollier, no improvement in the performance is observable in cases of congenital idiocy. The response appears, says Sollier, as though it were each time new to the infant.[82] As for the child without a cortex reported by Edinger and Fischer, it ceased to take the breast altogether during the sixth week of its life, and was thereafter fed with a spoon. But during the fourth month, while feeding it in this manner, the attentive mother noticed that the child made slight movements of sucking, which suggested that she should try it with a bottle. This proved successful; moreover the child would suck the bottle only when there was milk in it.

Whether a normal infant seeks the breast from the start is uncertain. But it is unable to find the nipple without assistance, though it succeeds in doing so after a few days, probably with the aid of smell—at least that is the only cue one can think of in the case of congenitally blind dogs, in which this capacity has also been observed. On an approach to the breast, however, the

tactual sensitivity of the lips probably also plays a part.

At first view, suckling seems to be a reflex-action. It takes place, in the beginning at least, as a reaction to a stimulus; its course is quite regular, it belongs to the congenital dispositions, and it is eminently useful in the preservation of the species. A closer consideration, however, reveals several important differences from the reflexes. In the first place, suckling, as already noted, is a relatively complicated act; which, however, in view of the indefiniteness of the statement, is not a very important difference. But, in the second place, the relation of the response to its stimulation is in several respects different from that usually found in reflexes.

Suckl
Reflex

(*a*) The movement depends upon the stimulus in the sense of being adapted to it, not merely because the reaction proves to be objectively appropriate—as when the pupil contracts more to a strong light than it does to a weak light—but because the act of suckling is regulated directly by the formal characteristics of the stimulating object. Thus the position of the lips in suckling must be different according as it is the breast nipple, a rubber nipple, an adult's finger, or the child's own finger, which is being sucked.

(*b*) Fine differences in the stimulus-complex may lead to opposite reactions (sucking or rejecting the nipple). These differences are sometimes of biological importance —for instance, the proper constitution of the milk to be taken.

(*c*) Aside from fatigue, the operation of a stimulus alone is not a sufficient condition for the appearance of the reaction. In addition, there must be a particular state of the organism as a whole—in this case a want of food; for we observe that the satiated infant no longer sucks, but rejects the nipple. Characteristic as are these differences, they would scarcely have sufficed to distinguish a special group of movements from the

reflexes, were it not that certain modes of behaviour have been discovered in the study of animals which originate neither in experience nor in deliberation. These are called *instinctive* movements, and suckling can be assigned to this group.

It will serve our present purpose to mention a few typical instinctive actions of animals.[83] A chick which has just broken from its shell pecks at any small object in its neighbourhood. This action requires no example from the hen or from another chick. Chicks hatched in an incubator act in this respect like those hatched in the natural way. The chick pecks only at objects of a certain size, such as grain, caterpillars, etc., that chance to be within its reach; otherwise it pecks without distinction and with surprising accuracy. This complicated movement is perfectly developed within a short time, though at first the defect may be noticed of pecking a little to one side of the object—missing it, however, only by a hair's-breadth. On the whole, pecking affords an instance of an extraordinarily precise co-ordination of optical stimuli with impulses that control a large group of muscles.

Another example is this: Birds that have been reared in an artificial nest, without parent birds, begin building their own nests when brooding-time approaches. For this purpose they employ every kind of suitable material, even including some which is not available under natural conditions, such as cotton wadding, coloured woollen, etc. The nest resulting from the use of these materials has, however, a form that is *typical for the species of bird*. Thus the swallow builds a different nest from that of the thrush. Without ever having seen a nest, and without an opportunity to imitate the nest-building activities of others of its kind, a swallow reared under artificial conditions constructs the same kind of nest built by swallows that have grown up in freedom. It is unnecessary to emphasize the fact that we are here dealing with a very complicated per-

formance. The nests of birds are often true works of art, as is shown in the case of the reed-warbler, which, building its nest in the reeds, must make it deep enough so that the eggs will not fall out even when the wind bends the supporting reed to the water's edge.

A third and final example may be given of a squirrel taken from its hole high up in a tree immediately after birth, and thereafter reared under artificial conditions. At first the animal was nourished with milk and biscuit, but one day it was offered a nut—the first it had ever seen in its life. The squirrel examined the nut attentively, and then gnawed around it until the kernel was exposed and devoured. Afterwards, whenever the squirrel was freed in the room it was observed that if more nuts were about than the animal could eat at one time, a nut would often be seized and cached. The animal would first look carefully around the room, and then run to some protected place—behind a sofa leg, or to a cavity in the foot of a carved table—and place the nut in the chosen spot. This behaviour terminated with the movements characteristic of burying a nut, and also of pressing the earth firmly over it. The squirrel would then go about its usual affairs quite undisturbed by the fact that the nut might still be wholly exposed to view. In order to comprehend this behaviour one must know that under natural conditions squirrels do actually conceal nuts in this manner. They hide their nuts two or three centimetres under the ground, recovering them later by the aid of smell. But the animal whose actions we have been reporting had never in its life been upon the open ground.[84]

These examples are typical of instinctive activities, and demonstrate that a living being can behave in a manner peculiarly suited to its own existence, or even necessary to the perpetuation of its species, without any relevant experience whatsoever. These acts are never quite simple—being for the most part extremely complicated—and the relation of the activity to its stimulus

is not at all simple. As demonstrated by the nature of the behaviour when it takes place under conditions very different from those of the normal habitat, the result of the action must be entirely unknown to the animal, and yet the animal *works towards a definite end*, and ceases only when this end—in so far as the conditions will permit—has been achieved. For example, the hen stops pecking when hunger is satisfied, and the squirrel stops scraping when the nut is buried. It is quite impossible to interpret these examples as evidence against the conclusion we have reached, by saying, for instance, that the squirrel is really quite unconcerned about the end, and merely performs a series of movements determined once and for all, which cease as soon as the series has run its course. That would be a quite unjustifiable generalization drawn from the squirrel's behaviour under artificial conditions and then applied to the animal's behaviour under normal contions. Since the nut can not be buried in the room, the usual achievement is impossible. But in the open country it is certainly not one and the same series of movements that lead always to the same end. How the squirrel must dig depends upon the nature of the soil, and its scratching and scraping must be different in firm and in loose earth, in dry and in damp earth, etc.

None of these activities is simple and all are extensive movement-complexes. Think how many and how varied are the movements requisite to nest-building; yet all are adapted to the surroundings just as suckling is adapted to the reception of nourishment. It is activities of this kind—found in their most complete form in insects—which we term *instinctive*. But we must be careful not to take this name as being an explanation of the behaviour itself. It is far too easy to believe that a real explanation can be avoided by merely labelling the action *instinctive*. Surely the term *instinct* removes from these activities nothing of the

mystery or incomprehensibility which they arouse in the mind of an unprejudiced observer; and from this point of view science must admit that instinct is still an unsolved puzzle.

We now see why we did not classify suckling with the reflexes, but instead placed it among the instincts; for instinctive action furnishes criteria which differentiate suckling from the reflexes (p. 89), both in its relation to a previously unknown achievement, as indicated by seeking and rejecting, and by the presence of extensive movement-complexes.

§ 7—*Instincts as Chained Reflexes. Thorndike's Theory*

When we were trying to explain reflexes in the preceding paragraphs (p. 70), we asked ourselves how an organ must be constructed in order that its function might be reflexive. We now ask the same question in regard to instinctive movements. How can we conceive a mechanism for the instincts?

The answer to this question promises to be much more difficult than our answer in the case of the reflexes—and, indeed, there is no universally accepted theory of instinct. Many investigators have given up attempting to explain it, finding in instinct an unsolved, and perhaps an insoluble riddle (Stern). One answer has been given to the question so frequently, however, that we must pay attention to it. This answer is that instinctive action is nothing more than a series of reflex-actions; more specifically, instincts are chained reflexes. A stimulus excites a reflex-movement starting the instinctive action. This movement either acts as the stimulus for a new movement or else occasions new stimuli from without which act upon the individual and in turn excite new movements. So it goes until the instinctive action is complete. For example, the hungry lion sets out upon a hunt for prey. The organic processes of hunger touch off the movements of search

for the prey. The lion begins to stalk the prey as soon as he is made aware of its approach by one of his sensory organs. He springs upon the hunted animal as soon as he comes near enough to it; and finally, he begins to devour it as soon as his claws and teeth touch it. Thus each movement leads to the arousal of a new stimulus which in turn excites a new movement. We have taken this example from the vivid account of William James, who, among psychologists, was one of the chief supporters of this point of view,[85] which originated with Herbert Spencer. The view is supported to-day in comparative psychology by the behaviourists, as we have already seen. In Watson's book entitled *Behaviour* we read that "an instinct is a series of chained reflexes." [86]

The same view is presented with remarkable cogency by Thorndike, who applies it throughout in his psychology of human development. Our consideration may therefore be based upon his statement of the theory. Thorndike teaches, as does the behaviouristic school in general, that every act of behaviour is a reaction to a situation, and that the act consists of three component parts: First the situation, within as well as without the body, which stimulates the individual; secondly, the reaction, a process within the individual which is a result of this stimulation; and lastly, the bond which makes this connection between the situation and the response possible. This, however, is nothing more nor less than the reflex-scheme as we already know it (see pp. 70 f.); though it has undergone a notable extension in the scope of its application so that it covers all acts of intelligence.[87] We shall consider this extension later; for the present we have only to deal with inherited modes of behaviour. These are characterized by the fact that the connection between situation and response is determined unequivocally by the order and arrangement of the neurones. One sees, therefore, that from this point of view there can be no form of inherited behaviour Thorndike

that is essentially different from the reflexes. With this in mind, it is clearly inappropriate to describe the instincts by reference to the ends they serve. Instead, one ought to characterize them with reference to the stimuli which call them forth. To attribute an instinct of self-preservation to an animal is just as inappropriate as if one were to attribute to oxygen an instinct to produce rust.

The mechanism of an instinct is therefore regarded as a system of reflex-arcs (see p. 71), although it has not yet been explained why the instinctive acts have so close a relationship to the ends they serve; for it was precisely here that we found the chief characteristic which distinguishes them from reflex-movements.

We may now add a few words to the statement already made on this subject. When we compare different situations which give rise to the same instinctive activities, we find that the alterations of behaviour, corresponding to differences in the situation, are of such a nature as to secure the same result in a manner conformable to the changed conditions. In carrying a heavy piece of building material to its nest, a bird must make other movements than in carrying a lighter piece. Such modifications of behaviour may take place easily and immediately. On the other hand, it may be that the original movement is first carried out, and, if it proves inappropriate to the new situation it may then be altered, and the alteration continued until the result is attained—excepting, of course, cases where attainment is impossible. An example of this method of procedure is found in the act of suckling; for if the milk-bottle is stopped up, suckling becomes stronger and more energetic. This peculiarity of instinctive movement is of the greatest importance. Lloyd Morgan has termed it "persistency with varied effort."

Again we can quote the views of one who will not be suspected of mystical tendencies. I refer to Tolman,

If the act is completed, these nervous pathways which are in a state of readiness actually become functional; but if the act is not completed, they remain inactive. Thus the conclusion is reached that a functioning of neurones in a state of readiness is satisfying to the system of neurones involved—or, as Thorndike says, to the *conduction-unit*—whereas not to function, when in a state of readiness, is annoying. There is also the opposite state in which a pathway may be either unready to conduct, or in such an unfavourable condition that conduction meets with resistance, so that when forced to conduct, the act is likewise annoying.

Thorndike is now faced with the problem of applying these laws to all situations which are originally satisfying or annoying. We shall not follow him in his task, which involves many hypotheses, but will proceed at once to estimate what has been gained by his theory for a solution of the problem of instinct, and especially its characteristic "persistency with varied effort." A principle already mentioned aids Thorndike at this point; the situation is supposed to release not one reaction alone, but a large number of different reactions. If the first act does not achieve the end but, instead, produces annoyance, then other possible reactions will be released by this failure and by what remains of the original situation, so that finally satisfaction is obtained; unless, to be sure, fatigue sets in and the animal abandons the attempt. This principle is found to be applicable in the explanation of "varied effort," because variation is attributed to the annoying situations, while cessation is attributed to the satisfying situations.

Two points are to be noted in this attempt to solve the problem of instinct.

1. In the first place, Thorndike's theory provides that, so long as unsuccessful movements are being made, they are always succeeded by others, determined by the situation and by the interconnections of the

neurones, until finally an end is achieved. Yet the substitution of one movement for another is in no wise determined with reference to a goal, but altogether by the neurone-connections laid down in the organism. The theory is, therefore, *mechanistic*, in the sense explained above (pp. 71 f.). The question immediately arises : How can a movement follow upon another that is unsuccessful ? According to Thorndike the answer would be this : The peculiar annoyance arising from an act, together with the remainder of the old situation, creates a new situation which releases a new set of reactions. But we have here the same difficulty we met with in the case of eye-movements ; for apparently there must be an infinite number of connections. Let us see how Thorndike himself describes the behaviour of a hungry kitten confined in a small cage with food in sight outside the bars. Having never been placed in such a situation before, the kitten " tries to squeeze through any opening ; it claws and bites at the bars or wire ; it thrusts its paws out through any opening, and claws at everything it reaches ; it continues its efforts when it strikes anything loose and shaky ; it may claw at things within the box. It does not pay very much attention to the food outside, but seems simply to strive instinctively to escape from confinement. The vigour with which it struggles is extraordinary. For eight or ten minutes it will claw and bite and squeeze incessantly." [90]

To ask what is the stimulus for this response, and to expect an answer in terms of the total situation, including the states of readiness in the neurones whereby the stimulus is supposed to release movements according to predetermined inherited pathways, seems a wholly inadequate statement of the case.

Hans Driesch and Erich Becher suggest other considerations.[91] The situation which releases the instinctive action is frequently of such a character that it may be resolved at different times into quite different

who writes as follows: "One observes that the (organism) *persists* through trial and error *until* a given *end* is *got to* or *from*. Such a purpose is quite an objective and behaviouristic affair. It is a descriptive feature immanent in the character of the behaviour *qua* behaviour. . . . It is *out there in* the behaviour; of its descriptive warp and woof."[88] I can think of no better expression for this side of instinctive activity. Yet it must be noted that it transcends radical behaviourism, just as Tolman's definition of behaviour transcends this doctrine as it is usually stated. Tolman observes what we have called *conduct*; the full profile of instinctive activity reveals itself only in the observation of conduct.

Thorndike tries to construct his theory so that it will embrace this feature of instinctive behaviour. The chief problem, as he sees it, is to find out why the reactions vary in the same situation, and cease only when an end is attained. But from the point of view of the reflex-arc theory, two different problems are here involved. One might explain variation by the application of Thorndike's hypothesis that the reflex-arc is not a simple mechanism, since the centripetal branch is connected in varying degrees of intimacy with numerous centrifugal branches, so that, in point of fact, different reactions corresponding to different connections function successively. Further assumptions are, of course, necessary to cover the serial order of the different movements; but the explanation is still incomplete, for why does the alteration of movement take place in the direction of a special consummation, or end?

Here Thorndike advances a new hypothesis. We might suppose that, as long as the end is not attained, the stimulus persists and continues to be effective. It must be explained, however, why the stimulus does not always call forth the same reaction until exhaustion sets in, instead of finally leading, through varied

efforts, to a successful response. Thorndike assumes here, as a part of the inherited disposition of the organism, that certain conditions are tolerated without opposition, or are even actively supported and maintained, whereas other conditions are naturally avoided or modified.[89] These conditions he calls "original satisfiers" and "original annoyers." As examples of "original satisfiers," he cites: "To be with other human beings rather than alone"; "To rest when tired"; "To move when refreshed." As examples of annoyers, he gives, "Bitter substances in the mouth"; "Being checked in locomotion by an obstacle"; "Being looked at with scorn by other men."

Since a collection of examples, however complete, affords less understanding than a law from which these examples can be derived, Thorndike formulates his law in the following terms: "For a conduction-unit ready to conduct, to do so is satisfying, and for it not to do so, annoying," which, however, only brings us back again to our original problem; for the result it was intended to explain appears again in the explanation. Unless we argue in a circle, this explanation is tenable only by recourse to the behaviour of the neurones. A situation may release a number of movements which are completely determined by inherited disposition. It is a function of this inherited disposition, however, not only to release movements, by conducting the nervous impulse over pathways, but also to set other pathways in readiness for conduction when their time comes. And hence, something is still wanting before James' example of the lion has been completely accounted for (cf. pp. 93 f.). If the lion is stimulated to stalk its prey by the scent of the animal, the chain of neurones that will later regulate the act of springing upon the prey must at the same time be set in readiness. Likewise, the system of nervous pathways upon which the still later activities of rending and devouring are dependent must in some degree be aroused at the very beginning of the hunt.

If the act is completed, these nervous pathways which are in a state of readiness actually become functional; but if the act is not completed, they remain inactive. Thus the conclusion is reached that a functioning of neurones in a state of readiness is satisfying to the system of neurones involved—or, as Thorndike says, to the *conduction-unit*—whereas not to function, when in a state of readiness, is annoying. There is also the opposite state in which a pathway may be either unready to conduct, or in such an unfavourable condition that conduction meets with resistance, so that when forced to conduct, the act is likewise annoying.

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Hans Driesch and Erich Becher suggest other considerations.[91] The situation which releases the instinctive action is frequently of such a character that it may be resolved at different times into quite different

stimulating elements, and yet, taken as a whole, the result remains the same. This is illustrated by the following example. Spiders of a certain kind possess an instinct which causes them to flee from bees, and they do so at the very first sight of a bee. Now Dahl has shown that no particular colour, odour, or size serves as an effective stimulus for these movements of flight on the part of the spider. Although the bee is unequivocally defined as a real object, it is not defined as a stimulus producing a definite retinal image; because the bee appears differently when seen from the front, from behind, or from the side, and also differently from above and from below. The effective elements of stimulation must therefore differ in accordance with the position the bee occupies with respect to the spider. Yet movements of flight are released even when the bee occupies the most unusual positions. Here we have an endless number of possibilities of stimulation by the same object; consequently, if the instinct-apparatus is conceived as a system of predetermined paths, these pathways must be almost infinite in number. Just how this problem can be solved is, of course, another matter, but the significance of this objection to the Spencerian theory of instincts can scarcely be denied.

2. On the other hand, Thorndike's theory is a positive contribution to the subject, inasmuch as his doctrine of satisfying and annoying situations furnishes the nucleus of a solution by the support it gives to a principle that can be expressed in the following terms: Physiological processes take place both in the form of "closed" and "unclosed" responses. This principle appears in Thorndike's work only in a special form which is closely interwoven with all his other assumptions, but it is a principle of the greatest significance in the explanation, not only of instinctive acts, but of behaviour in general.

§ 8—*A Contribution to the Theory of Instinct, looking towards the Abandonment of the Alternative Views of Mechanism and Vitalism. Instincts and Reflexes*

The Spencerian Theory, taken over by Thorndike, is entirely inadequate without a reinterpretation of behaviour in terms of "closed" and "unclosed" physiological systems; but even with this it still has certain incurable defects. We must try, therefore, to understand instinctive action without the hindrance of any theoretical presuppositions. It will be helpful to distinguish between instinctive and reflexive actions even more closely than we have already done. We found that the concept of reflex-action harmonizes with the hypothesis of a simple reflex-apparatus. Partly following Stout we can now supplement our earlier statements in three ways.

1. A chain of reflexes must consist of a number of separate part-activities determined in a purely external manner by the order of the system of neurones laid down in the organism. If we name these single part-activities *a*, *b*, *c*, . . . then *b* is carried out because it is stimulated either directly by *a* or by a stimulus that becomes effective through *a*, and *c*, in turn, has the same relation to *b* that *b* has to *a*. In short, every successive part-activity stands only in relation to the immediately preceding activity, or to its effects. Moreover, if we accept Thorndike's hypothesis of readiness, the particular act may be conditioned by many or all of the preceding part-activities. We ought, therefore, to be able to alter the succession of movements in an instinctive act, if it were possible to alter the connections between the neurones that condition them. In other words, it ought to be possible by merely shifting the nervous connections to cause a hen to begin her pecking behaviour with the act of swallowing, instead of first thrusting forward her head and neck, then opening her

bill and seizing the grain. But when we consider the experiments of Marina, described in note *78*, in which such an alteration of nervous connections was actually carried out, we can have little doubt as to how our experiment would come out. Even if we could shift the connections we should not alter the succession of movements in the way required by the theory.

Furthermore, when we consider a typical instinctive action as it appears in the natural course of an animal's life, the impression is not at all that of a summation of part-activities which have in themselves nothing to do with one another. On the contrary, an instinctive activity takes a *uniform course*; it is a *continuous movement*; it does not appear as a multiplicity of separate movements, but as one articulate whole, embracing an end as well as a beginning. Every member of this activity seems to be determined, not only by its position with reference to what has gone before, but also with reference to all the members of the act—and especially to the last phase which leads to the result. An instinctive activity does not make an impression upon us like a succession of tones, for instance, which a playful child might produce by pressing the keys of a piano in irregular succession; but rather one like a melody. We can also describe the facts in this way: An instinctive reaction is *adapted* to its stimulus; it is not merely set off by it. And this truth applies not merely to the end, but to the reaction as a whole. We have already referred to the fact that an instinctive reaction modifies itself according to the situation which arouses it. Under certain circumstances, obstacles interfering with its progression are set aside, while the tendency persists, with varied effort and by varying means, until the end is attained. Consider the building of a nest. One can not say at any particular stage in its construction that the bird will now make this or that movement; one can say, however, that the bird must now fulfil this or that requirement.

I wish it understood that these statements are intended as an unprejudiced description, without theoretical presuppositions. The truth of our description can therefore freely be admitted, even though one sees fit to maintain that in reality the behaviour is something quite different. It is also significant that descriptions of this sort are suitable not only for instinctive actions but also for higher types of behaviour which we call acts of intelligence.[92] We shall, therefore, employ such descriptions frequently in what follows. Yet the reader need not hesitate to accept our description through fear of being led into a false theoretical conclusion; for, of course, one can not infer from this agreement between instinct and intelligence that an intelligent consciousness must participate in instinctive action—an inference which has been drawn, for instance, by Stout. On the other hand, it would be equally inadmissible to pass this similarity over and leave it out of consideration.[93]

2. While reflexes are typically "passive" modes of behaviour, which depend upon the fact that some stimulation has taken place, instinctive behaviour is, by contrast, significantly "active" in its search for stimuli. The bird *seeks* the material for its nest, and the predatory animal *stalks* its game. In other words, the stimulating environment is not a sufficient cause for these activities. Every movement requires forces which produce it; but the forces that produce instinctive activities are not in the stimulus-situation—they are within the organism itself. The *needs* of the organism are the ultimate causes of its action; and when these needs have been satisfied, the action comes to an end.[94]

3. Instinctive activity has also specific relations to the sense-organs. On the one hand, an instinctive activity is characterized by the particular stimuli of a certain sensory domain that arouse it. The stimuli

which affect one species most vigorously may leave another species untouched.[95] On the other hand, the situation which presents itself to the sense-organs, after a movement has taken place, determines the continuation of the movement; success and error are differentiated, so that varied activities may lead to a single goal.

From this summary it appears that the instinctive activities are much more like voluntary activities than they are like pure reflexes. At any rate, they possess the same forward direction that is characteristic of voluntary action.

One might object that such a forward direction could only be given if the animal already knew the goal towards which he was striving. In the case of voluntary action this knowledge is presupposed, but not so in the case of instinct, where the animal must direct his course without any previous knowledge of the end. How, then, is it possible to strive for a goal of which nothing is known? To this question Stout gives the right answer. One can quite well be directed forward without knowing anything of the goal which one is approaching. One can wait, and yet know not what one is waiting for. The present situation appears, therefore, not as one that is constituted thus-and-so, but as one that is constantly undergoing change. It is not a *state*, but a *transition*; not a *being*, but a *becoming*. There is no difficulty in comprehending what this means. From the first scene of a drama one may feel that something terrible is going to happen, and thereafter all that occurs on the stage is merely a preparation for, or a delay in, the tragic *denouement*; and yet one would not be able to tell what it is precisely which thus hangs, as it were, in the air.[96] As a simple example, suppose you are listening for the first time to an unfamiliar melody, which ceases abruptly before its termination; you will then have a very clear impression that the music should continue. Or again, if some one taps the following measure:

. .— . .— . . , the last beat has no finality; the rhythm ought to go on. In this instance, the expectation is quite definite, but in the preceding case it is not altogether indefinite, though the indefiniteness may under circumstances be greater than it could possibly be in the case of a simple rhythmical succession. Even in the instance of the drama, the tragic end which hangs over the audience is not altogether undetermined. Indeed, the definiteness of expectancy consists not only in the knowledge that a definite situation must change, but also in a knowledge of the direction of the change itself, no matter how indefinite this change may be. If the course of action be interrupted in any one of our examples, we have not merely stopped an external succession of independent processes; we have disrupted a unitary course of events which, though incomplete at the moment of interruption, yet bore within itself, and evolved as it went along, its own law of progression. Indeed, I should go further than Stout does; for I think it quite possible that this is a fair characterization of instinctive behaviour, so that one might say that the nearer an animal comes to the end of his instinctive action, the more clearly and definitely will the directions of change reveal themselves in the as yet incomplete present situation.

The process, or movement, of a melody is something quite different from mere succession. A true movement requires forces, and forces are vectorial magnitudes—that is to say, a force is always a force in a certain direction. In the cases discussed in the last paragraph the forces originated in the phenomenal field (though not in the stimulus !). The beginning of a melody pushes forward in the direction of its continuation and completion. Since instinctive activity arises from organic needs, it must be directed from the very outset. A feeling of hunger is a sufficient guide, without having first to learn that what one wants is food. However diffuse this feeling of hunger may be,

it leads to an activity which is very definitely directed towards food. The need of food corresponds to a certain stress, or tension, and the activity that follows relieves tension. Inasmuch as the activity is a result of this tension, it can proceed only in such a way as to relieve it.

In order to make clear this point concerning the "outer" and the "inner" behaviour of an animal when it acts instinctively, let us consider the nature of a human instinctive action. Suppose some one suddenly hears shrieks which betoken agony and distress; at once he will move in the direction from which the shrieks come, and if he finds the victim who uttered the cries, he will try to render him assistance in his trouble. What is this person's "experience" like from the moment he hears the shrieks? Undoubtedly it will be pity mixed with horror. Thus some other person in whose experience the second of these components prevails, instead of being drawn towards the sufferer, will be tempted to run away. The "inner behaviour" of these persons is therefore *affective*, and the phenomena which accompany their action are of the type called *emotional*. Furthermore, these emotions, this "inner behaviour," will fit the external behaviour of the instinctive act perfectly, just as our general conception of behaviour requires that they should. This conception of the relation between instinct and emotion has been developed by William McDougall, who writes: "Instinctive activity is naturally accompanied by some degree of a general felt excitement; this felt excitement, accompanying the operation of any instinct, is specific in quality to that instinct." And hence, running away we feel fear, striking out we feel anger, spewing out we feel disgust. In a recent paper, Lloyd Morgan has expressed similar views.[97]

I believe we can go a step further, and at the same time meet some of the objections that have been raised against McDougall's theory. We have attributed the

ultimate cause of instinctive activity to certain needs of the organism. During long periods of time these needs may lie dormant, being neither effective nor felt. Yet they can be aroused from their dormant state to activity, as our previous example of the effects of woeful shrieking has indicated. Under the circumstances described in this example, it seems to me that the first reverberation in the hearer's consciousness is the inner aspect of these needs themselves. Generally speaking, emotions not only reflect behaviour, they may also precede behaviour as a direct expression of needs which have become active. Other forces at the moment may inhibit the organism from behaving in accordance with these needs, although the emotion thus aroused is strongly felt. We cannot say, however, that the consciousness of the moment always reflects the dominant need. As the psychoanalysts have taught us, and as Lewin [98] has recently emphasized, the momentary consciousness may reflect some superficial or quasi-need, such as intention or resolve, whereas the general direction of one's total behaviour is all the time determined by more potent forces which are not projected into consciousness.

It may be objected that the responses attributed to a sudden shriek are not instinctive but acquired ; that their mode of execution depends primarily upon the individual experience of the agent, and the nature of his environment. This objection, however, overlooks a very important distinction. Instinctive activity can be defined by the forces which cause it. Activities are then called instinctive which are aroused by original instead of acquired needs. But instinctive activity, as usually understood and as it has been exemplified on pp. 90 f., possesses still another striking characteristic : The execution of the act itself seems to be largely determined by the original nature of the organism. Not only the need, but also the way in which the need is satisfied, appears to be inherited. This second

characteristic, which is not always clearly distinguished from the first, seems to be mainly responsible for mechanistic theories of instinct like the chain-reflex theory. We shall return to this point later. For the present, the distinction will help us in meeting the objection that the responses described in our example were not instinctive. Bearing in mind that we have not yet considered what learning means, we may grant freely that the activities involved in succouring someone who shrieks for help, or in running away from a terrifying situation, are more or less acquired. Still we can insist that these activities were instinctive, because they had their origin in fundamental needs.

Returning, then, to our main topic, we note the fact that, without starting from any hypotheses, the study of instinctive behaviour, itself, brings us to the same conclusion reached by Thorndike regarding the difference between a closed- or end-situation and a transitional situation. So long as the activity is incomplete, every new situation created by it is still to the animal a transitional situation; whereas, when the animal has attained his goal, he has arrived in a situation which to him is an end-situation. Thus the animal's phenomenal situation, with its articulation into more or less distinct parts, becomes dependent upon the animal's activity. The "functional" value of any part of the phenomenal field, whether it be a "starting point," a "transitional situation"—either as a means of attaining the goal or as an obstacle in the way—or the "goal" itself, each varies with the activities in which the animal is engaged. The "same thing" may be either a starting point or an end; although phenomenally the "thing" is not at all the "same" in these two cases. For instance, a high and rugged mountain does not look the same to a traveller who approaches it in order to get the view (final character), and to a mountaineer who is making preparations to scale its heights (initial character).[99]

Aside from the activity of the animal, the pheno-

menal field itself has dynamic character. Thus, the articulation of its parts may be "good" or "bad"; the parts may be "complete" or "incomplete"; the arrangement may be "closed" or "open." Laws of articulation are here indicated, which belong essentially to the phenomenal field. To illustrate: Fig 4 appears at once as an open triangle, although, being open, it does not have three angles. We might say that the figure exhibits "non-closure," yet indicates with a high degree of certainty the direction in which "closure" is to be effected.

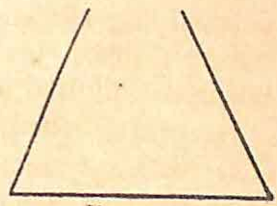


FIG. 4.

When we consider that conscious phenomena belong to our behaviour, just as all our behaviour is bound up with definite processes of the central nervous system, the conclusion to be drawn from instinctive performances is that the characteristics of "closure"—as we shall call it—belong not merely to the phenomena themselves, but likewise to the behaviour taken as a whole, including all reactions made to the environment. Instinctive activity then becomes an objective mode of behaviour analogous to such phenomena as rhythm, melody, and figure.

Now the question arises: How shall we conceive the apparatus of these functions? As our later chapters will show, and as modern psychology is daily proving, it is quite impossible to identify any scheme of chained reflexes with the device needed. But at this point arguments directed against the explanation of instinctive activity in terms of reflexes meet their counterpart in arguments which deny the whole issue as to an appropriate apparatus for these functions, and assert instead that the events of life can not in any way be traced back to the same laws that dominate the inorganic world. According to these arguments, the conclusion has been reached that the operation of a

specific " vital force " expresses itself in the events of life by means of forces which are either essentially mental, or, at least, conceived as being directly related to mind.

This view is called *vitalism* or, in so far as vital and mental energy are identified, *psycho-vitalism*. Köhler has justly remarked :[100] " If one asks what phenomena of experience prompt the vitalists to accept this view, it may be answered that the motive of many can be found in what we have termed ' closure,' both in the organism and in its behaviour."

In the first chapter of this book various objections to a " psychological theory " were set forth ; but despite all of these, if the choice lay between a (psycho-) vitalistic and a mechanistic explanation, we should be caught in a dilemma, one horn of which (vitalism) requires us to renounce our scientific principles, while the other (mechanism) implies an entirely false attitude towards life. This alternative, however, is not forced upon us—as Wertheimer was the first to make evident in his new theory of the brain-processes.[101] If nervous processes correspond to such phenomena as rhythm, melody, and figure ; and if the pathological cases, in which an injury to the brain renders the creation of such phenomena difficult or even impossible, teach us that nervous processes must have a share in occasioning them ; then these same nervous processes must embrace all the essential characteristics of the phenomena in question. Köhler, indeed, has demonstrated that formal qualities belong likewise to inorganic processes in quite the same way as they are evident in the phenomena we have mentioned.

Again I must confine myself to a few suggestions relative to this subject. We are confronted with two separate problems : (1) Is there any such thing as " closure " in inorganic processes ; and, if so, (2) Does " closure " exist in such a form that we can regard it as analogous to our distinction between an end- and a

transitional situation? The first problem is the more difficult of the two. Köhler has solved it first for processes which are independent of the time-parameter, namely, states of rest and stationary processes—events which do not alter their characteristics with the passage of time—as, for example, a constant electric current, or the flowing of water in a tube. He demonstrates that these states and processes do possess the features of "closure." The reader will not be able correctly to understand the significance of this solution until he knows more exactly what is meant by "closure." The meaning, however, will become clearer in the course of further discussion, while, at the same time, the significance of the solution will also become more evident.

A solution of the first problem leads at once to the second. Among the endless multiplicity of states and events, those called stationary are the states of "closure" in which all happenings find their issue. The distinction of these particular states of "closure" may be characterized in two ways: (1) they satisfy certain conditions of energy,[108] and (2) they possess a certain simplicity and compactness, which, in isolated cases, can be defined mathematically—though at present this can not be done in all cases. A concrete example will best explain what we mean. A soap-film is produced upon a wire-frame, and upon it a little noose of thread is cast in whatever form it may take. If one proceeds carefully, the thread will be supported upon the surface of the film, "but if one pricks the film *inside* the noose with a needle, the surface will break apart and the thread will be pulled out by the surface-tension of the outer portion of the film, which seeks to give the area outside the thread the least possible surface, and the area circumscribed by the thread the greatest possible surface. As a result, the thread immediately assumes the form of a circle." In this example we can conceive of circularity as the "end-situation," puncturing the soap-film as the stimulus

releasing the movement, and the movement itself as the "transitional situation." The same procedure holds true for all events, and especially for those that issue in the nervous system. Thus inorganic nature includes the possibility of "closed" events—at least in the case of events independent of time; and the distinction of the end- and the transitional situation is as appropriate here as it is in organic behaviour. This fact, to be sure, is not all that is necessary to explain instinct; because the unitariness of instinctive action very obviously suggests that with "closure" the whole temporal course of the activity is involved, and, so far, we have not referred to any dependency upon time. Yet this reservation raises no difficulty in principle. The same hypothesis which is applicable to stationary events can be carried over to the events of a dynamic series—although with much greater difficulty as regards details; so that even in the field of physics it can be demonstrated that dynamic processes also exhibit "closure." In psychology it was precisely the dynamic phenomena of seen-movement that furnished the starting-point from which this new hypothesis was developed.

§ 9—*Instincts, Reflexes, and Inherited Behaviour*

Behaviourism has influenced psychological theory far beyond the limits of the strictly behaviouristic systems; for it has succeeded in making two pairs of concepts the pivotal systematic categories of our science. These are the concepts of stimulus and response, on the one hand, and of original behaviour and habit, on the other. The second of these pairs will concern us when we discuss learning. An immediate consideration of the first pair will help us to develop our view of reflexes and instincts. At the present time there is a very general tendency to describe and explain behaviour by correlating responses with their stimuli. This correlation will explain be-

haviour, if behaviour is only a sum of simple reflexes, which is the postulate upon which the stimulus-response conception rests. The organism is supposed to respond to a stimulus, because it possesses sense-organs, nervous structures, and effectors of such a character that the stimulus will release an excitation of one or more reflex-arcs.

The response, according to this assumption, is a response to the stimulus in a very superficial and extrinsic manner. The real determiner of the response is the reflex-arc—the conjunction of nervous and muscular tissue which, at the moment of stimulation, is already pre-formed in the organism. The stimulus is a causal factor only in so far as it can determine which of many bonds of connection shall be made.

It will at once be apparent to the reader that our previous discussion of both reflexive and instinctive activity is in direct opposition to any such view of the matter. In discussing eye-movements, we found them to be regulated by the visual phenomena themselves, or at least by the physical correlates of these phenomena (cf. p. 82), and the same is true of all instinctive activities. Using the term "receptor-process" to designate a physiological process in the sensorium which follows the stimulation of a sense-organ—whether or not this process is accompanied by conscious phenomena—we can then give a generalized description of behaviour as follows: "Our direct responses to stimuli are receptor-processes which in many cases will be at a mental, or perceptive, level. Such a direct response is, however, only the beginning of a total response. The perception issues in action according to its constitution. The action is a natural continuation of the perceptive process; it is determined by it, and not by pre-established bonds of connection." [103] Whenever action is on the perceptive level we can also say that the organism reacts to things and events which constitute its phenomenal world, in a certain stimulating situation.

For instance, I react to a pretty face, and not to the rays of light which this face, as a physical object, reflects into my eyes. From this point of view the process of behaviour is intelligible, as it is not from a mechanistic point of view; because if we speak in terms of stimulus and response, we have no right to ask why a certain stimulus produces a certain reaction. We must be satisfied with the facts of the case. A so-called explanation in terms of bonds of connection between neurones is at best an additional fact; it is not an intelligible reason for the reaction based upon the nature of its stimulus.

Even the explanation which we have offered for reflex-activity derives the response from intrinsic properties of the phenomenal field, or receptor-processes. The validity of such an explanation has been shown by various examples, notably those of eye-movements. Still another example will show how we must regard the development of reflex-activity from this point of view.

It has been pointed out (p. 73) that in the first days of infancy blinking does not occur when an object is moved quickly towards the eyes. This failure of the blinking-reflex has hitherto been explained by the statement that at birth the necessary nervous connections are not yet permeable, and the occurrence of the reflex must therefore await the ripening of these nervous structures. But we are now able to offer a quite different explanation: At first the quickly approaching object fails to arouse the specific receptor-process because in early life rapid movement is not visually perceived (cf. p. 65); and, accordingly, the corresponding protective reaction does not follow. From this example it appears that the development of the reflex takes place in the receptor-process.

If reflexive and instinctive activities are made intelligible by deriving them from certain essential properties of the phenomenal or receptor-field, are they

not at the same time made *intelligent* acts? My answer is: Yes, but I do not regard this as a valid objection to the theory. Certainly, the so-called original acts of behaviour look more intelligent than the reflex-mechanisms which many psychologists have been wont to substitute for original behaviour in an effort to explain it. A response that owes its nature and direction to certain rigid external constraints is surely less intelligent than one that results from the spontaneous distribution of forces in the field. However intelligent the inventor may be, a calculating machine is less intelligent than the so-called system of reflexes which controls eye-movements. A reflex-system will act in accordance with the forces in the field even if the conditions which limit the freedom of the system have been altered.[104] The "system" is therefore intelligent in a way in which a calculating machine is not; because when two connections of the machine are interchanged all the sums will be wrong.

Although I do not hesitate to attribute intelligence to reflex-activity, the distinction usually made between instinctive and intelligent behaviour still has its importance. The relation between instinct and intelligence will be discussed later on (pp. 242 f.), but the way to this discussion can be opened now.

We have more or less neglected the characteristic rigidity of instinctive activity, which is chiefly responsible for the development of the chain-reflex hypothesis. Although this characteristic has been both exaggerated and misunderstood, the term rigidity, though inadequate, refers to certain very definite features of instinctive behaviour. We have already distinguished two aspects of instinctive activity, namely the fundamental *need* for action and the inherited *way* in which the need is satisfied. The rigidity of instinctive behaviour refers to the execution of acts which are determined by the organization of different species of animals. The behaviour of the squirrel, described on p. 91, was not

in accordance with the artificial environment of the room in which the nuts were presented, but, instead, followed the route it would normally have taken in the natural surroundings of the forest. It is for this reason that the squirrel's behaviour appears to be stupid. Another, and a different, case is reported by H. Volkelt [105]: A spider rushes out of its nest and attacks a fly which has been caught in its web. The fly is killed and fastened to the web; after which the spider returns to its nest and proceeds to finish its meal upon another fly whose remains are there. Only after its meal is finished does the spider return to the web and bring the new-caught prey into its nest. On the other hand, when the experimenter introduces a living fly directly into the nest, the same spider, instead of attacking it, reacts to it with the behaviour of avoidance and flight. This behaviour appears also to be stupid; but it is in one important respect different from the stupidity of the previously described squirrel. In the case of the squirrel we have normal behaviour under abnormal conditions which do not warrant it. In the case of the spider normal behaviour is lacking under abnormal conditions which do warrant it.

Are these two kinds of stupidity compatible with our theory? Let us begin with the spider. If the spider's motor response is a consequence of its receptor-process, the reaction of flight instead of attack must indicate that the functional character of the fly, when artificially transported from web to nest, has changed from "quarry" to "dangerous foe." Since the same physical object—a fly—can occasion these two quite different functional impressions and corresponding responses, it follows that the articulation of receptor-processes in the spider may be such that the same element is not identified in a different environment. This fact, of course, is a common-place, not only for spiders, but for human beings. What is specific to the spider is the degree and kind of dependence of the element upon

its environment. Speaking in anthropomorphic terms, the spider's stupidity consists, not in running away from a frail victim, but in mistaking its victim for a formidable foe. Employing the same common-sense terms to describe the stupidity of the squirrel, we might say that it consists, not in trying to bury a nut in the hard floor, but in mistaking the floor for soft ground. These two mistakes, though different, have one trait in common: In each case the receptor-processes were inadequate to the stimulus-situation. Either the receptor-processes varied too little with the situation—as they did when the squirrel perceives the room as if it were the forest—or they varied too much with the situation—as they did when the spider perceives the fly as a different being in two vitally different surroundings.

Coming now to the question of rigidity of behaviour, only the first kind of mistake indicates what is called rigidity of instinctive activity. The less closely the phenomenal field follows changes in the physical field, the more will the animal's action appear to be machine-like and independent of the situation. In reality, however, it is nothing of the kind; for the animal still reacts intelligently to what it perceives, although its perception may be entirely inadequate to the real situation. The fact that the form of instinctive activity distinguishes different species of animals shows that, subject to individual variations, each species possesses a certain kind of sensorium which determines the kind of receptor-processes of which the animal is capable.

Is a true distinction between instinctive and reflexive activities possible from this point of view? Certainly, the distinction is not easily drawn. Compare, for instance, the "reflexive" eye-movements of the human infant with the "instinctive" pecking of a newly hatched chick. The performance of the chick is the more complicated of the two; and yet both are regulated by the optical system in attaining their ends.

The similarity of the two performances becomes even greater when we take into account the movements of the infant's head which, as a rule, accompany the movements of its eyes. We might, indeed, have chosen many other examples of this similarity; for there are numerous instances where one is in doubt whether the behaviour should be classified with the reflexes or with the instincts. Thus a pheasant, just hatched from the shell, its bill smeared with the white of the egg, will proceed deliberately to wipe its bill on the ground.

When we consider the executive aspects of these unlearned acts, it appears to be impossible to draw any hard and fast line between reflexive and instinctive activity. In extreme cases, however, certain differences are manifest. Although a reflex is never a simple event, to be fully accounted for by a single reflex-arc, the typical reflex is a more isolated reaction than the typical instinctive act. The precision and regularity of reflexes are more largely determined by pre-formed structures, than instinctive acts. The general theory of a spontaneous distribution of forces might enable us to explain why a young chick pecks, and how its pecking eventuates in food-getting; but the remarkable speed and accuracy of its behaviour requires the assumption of pre-formed structures.

If the marked contrast between the stereotyped character of certain reflexes and the variability of certain instincts, like those which determine the behaviour of a wasp or a spider, is to be regarded as a difference of degree and not of kind, we must raise the question: How do reflexes become fixed mechanical types of work which suggest a mechanistic theory? Instead of answering this question in full, we are unable to do more than suggest a direction in which an answer may eventually be found.

In addition to the reflexes themselves there are many other modes of behaviour that possess a reflexive

character. These are the so-called automatic activities —habitual movements which have been termed "acquired reflexes," though originally they were not automatic but voluntary acts which only became automatic as a result of frequent repetition. Since their quasi-reflexive character was acquired in this way, we can perhaps assume a similar relationship between the true reflexes and the instinctive activities. If the so-called acquired reflexes can be conceived as fixed voluntary acts, perhaps the pure reflexes are likewise conceivable as a result of instinctive fixation. It is noteworthy that Erich Becher—who rejects a mechanistic theory of instinct, in favour of a psycho-vitalistic theory—adopts this interpretation of the reflexes, and tentatively employs for the reflexes the same principle which he has elaborated in explanation of the instincts.

More definitely expressed, what we suggest is that a normal function may give rise to relatively fixed anatomical structures which, on the one hand, decrease the freedom of the entire system by subjecting it to constraints, and, on the other hand, may only be of such a nature as will promote the normal function with a greater degree of accuracy and promptness. The result of such a development may be called a "quasi-mechanism." Since, however, such a mechanism owes its existence to a circuit of events similar to the one previously described in the case of eye-movements (p. 83)—where the function leaves traces that again influence the function itself—it follows that the development of a "quasi-mechanism" requires no other explanation than that which applies to any other development of behaviour.

This explanation reverses the usual view that instincts are derived from reflexes, and at the same time shows how the function of a reflex can aid in its development as a stereotyped behaviour. Indeed, it is the function which creates the structure—a conclusion which has also been reached by certain leading

biologists.[106] We can therefore take our stand with respect to one important question, and conclude that the kind of explanation which we found applicable to eye-movements must in principle be applicable to all other reflexes. It also appears that, strictly speaking, reflex-action is not a fundamental form of behaviour, but has developed out of a form much less constrained.[107]

There is, however, still another way of distinguishing reflexive from instinctive activities, which does not depend upon the executive character of the behaviour. If we consider the forces which occasion the response in each case, we find, as has already been noted, that instinctive activity springs from the fundamental needs of the organism. The phenomenal or receptor-field, important though it is for the execution of the activity, does not itself contain the forces which excite the animal's behaviour. In the case of a strictly reflexive activity no such needs are demonstrable. Here the forces lie in the receptor-field. Thus, a lack of balance in the visual field forces the eyes of an infant to fixate a light in the dark; or a lack of simplicity in its auditory field forces the infant to turn its head towards the source of a sound. Here, then, in terms of the forces which occasion activity, we find a true difference in process, which is far more significant than the usual basis of classifying reflexes and instincts.

We are now ready to ask a final question on this subject: In what sense are reflexive and instinctive activities inherited? We are no longer in a position to correlate any reaction with any specific device of the nervous system. The same reaction may take place with a changed nervous structure, and the same nervous structure can give rise to different responses. Each response is now conceived, more or less, as a function of the total organism. According to this conception a response taken as a *sum* of the single reactions of each part of the organism is only one among numerous possibilities; and this one is *per se* highly improbable.

In order to make this point clear, an analogy may be drawn from physics. The intensity of an electrical current is proportional to the electromotive force, and to the conductivity of the system. To every combination of these two magnitudes there corresponds an electrical current of a definite intensity. But it would be unreasonable to ask how much of this intensity is attributable to the electromotive force, and how much to the conductivity of the system. The same unreasonable type of question seems to be asked of inherited behaviour by many theorists who demand that one part of a reaction shall be ascribed to one part of the organism, and another to another. These theorists do not seem to consider that this demand may be just as meaningless as the corresponding demand for a division of an electrical current. The fact is that the relation between a set of conditions and the process which takes place under these conditions is not as a rule such that we can divide the total process into a finite number of part-processes each of which will depend upon a certain part of the conditions. K. Goldstein [108] has clearly proved that nervous processes in particular do not permit of this kind of analysis. I myself, have reached the following conclusion: On account of his psychophysical structure an individual possesses certain properties. These properties, together with his external social and physical situation, constitute the conditions of his behaviour. When the individual makes a certain reaction *a*, it is unnecessary to assume for it a special mechanism *A*, because the same structure of the individual which in whole or in part occasions reaction *a* under one set of conditions may occasion a quite different reaction under another set of conditions.[109] What we inherit, then, is not a repertory of particular reactions, but a set of internal conditions for response, which together with external conditions, physical and social, co-determine our behaviour. If we inherit anything specific, it is certain needs or stresses which pull

us in certain directions. These needs or stresses result in responses which greatly vary with the external conditions under which the behaviour occurs, yet remain constant in the direction of satisfying the needs which gave rise to them.

§ 10—*The Instincts of Newborn Infants, with some General Remarks upon the Instincts of Man*

After this long theoretical discussion, we may now return to a consideration of the instinctive movements of newborn infants. The most striking thing to be noted is that the infant makes very few movements, and very few well-developed serial activities, which can be called "instinctive." Stern [110] singles out from among the activities of newborn infants an instinctive "attraction" which draws the child towards different stimuli from the very first day of its life. Thus, an infant whose cheek is touched with the finger quickly turns its head in such a way that the finger is brought into contact with its mouth. Even on the third day after birth, before any actual contact had been made, the nearness of the mother's breast exerted this attraction in the case of Stern's oldest daughter—the stimulus apparently being based upon sensitivity to odour. Similarly, intensive light-stimuli will cause the head to be turned in the direction of the light. As we have already seen, all these movements, and particularly the last one, are closely related to eye-movements. Likewise the opposite reaction of avoiding a "negative" stimulus is conditioned from the start, as Preyer observed in his son on and after the fourth day whenever the left breast, which the child found difficult to nurse, was offered to him. It is hard to decide which of the two conditions for original behaviour these responses fulfil. Are they the result of fundamental needs aroused by the present situation, or are they simply the result of forces operating in the receptor-field? Even the addi-

tion of other movements, with which we shall become acquainted in the following section of this chapter, supplies as an inventory of the instincts functioning from birth only a very paltry list as compared with the instincts of many animals standing much lower in the scale of development. "The really pitiable helplessness of the newborn human being is accounted for by a dearth of ready-made instinct-mechanisms," says Bühler;[111] and this is correct, save that we should not employ the term "instinct-mechanism."

The conclusion that man, in a general way, possesses fewer instincts than any other animal has, however, been disputed. James in particular has tried to demonstrate the contrary. In order to understand how one can entertain James' position, it is necessary to distinguish between original needs, and original ways of action. If we refer to fundamental needs, man has many instincts; but if we refer to stereotyped ways of action, man has very few instincts. The ways in which fundamental needs are satisfied can be largely modified by experience; and the more capable an organism is of learning, the more ways it will find in which to act. Since of all living creatures man possesses the greatest capacity of learning, it follows that his ways of satisfying his needs will be much more variable than the ways of any other animal. If one thinks of instincts as rigid types of behaviour, one will find few if any of these in man. On the other hand, the fundamental needs of man will influence his conduct quite as much as such needs influence the behaviour of animals lower in the scale of life.

A few examples may be added in order to indicate the kind of modification which original responses may undergo.

Chicks just hatched from the shell will peck at all sorts of objects within reach, provided these are of a certain size. Hence, if one place before the chick a cinnabar caterpillar, which is readily distinguishable by

vision on account of its alternating bands of black and gold, the chick will at once peck at it. But the caterpillar is immediately rejected, and the chick wipes its beak as a token of disgust. If the experiment is repeated after an interval of, say, one day, most chicks are already disposed to inhibit pecking, and the caterpillar is not attacked.[112] Lloyd Morgan has fully described this transformation of an instinct, which may take place after a single experience. The same investigator has also observed that young birds learn in this way to avoid pecking at their own fresh excrements.

Another example can be given from a much lower stage in the animal series. It is well known that stereotyped modes of behaviour, called *tropisms*, can be observed in lower forms of life. These may be briefly characterized as a positive or negative behaviour with respect to certain stimuli; some stimuli are sought, while others are avoided. A cockroach possesses a negative photo-tropism, that is to say, it avoids the light, and makes its abode in dark places. The experiment was made of stimulating a group of these insects, gathered together in the dark, by an electrical shock; the result was that the insects congregated thereafter on the lighted side of their cage. But the original tropism was not on this account necessarily annulled, any more than a chick loses its pecking instinct after an unpleasant experience with a cinnabar caterpillar, and, indeed, when the insects were removed to another, differently constructed cage, they again took up their position on the darker side.[113] Tropisms, therefore, are subject to modification even with organisms quite low in the scale; so too are reflexes, but only after a painstaking process of "conditioning."

For the present this is about all we have to say of human instinct, although the problem of instinct and experience, to which Lloyd Morgan has devoted an entire volume, is overflowing with questions of detail which, if space permitted, could be profitably considered.

The reader will find valuable data upon this subject in the works of Thorndike and McDougall, as well as in the book of Lloyd Morgan just referred to. In addition, what James has to say of instinct is so vigorously put that, although the fundamental differences between his point of view and the one here supported are extreme, the reading of his chapter is nevertheless to be highly recommended.

One peculiarity of instinct should be mentioned, however, upon which James placed great emphasis. This is the so-called transitoriness of instincts. Many instincts, i.e., fundamental needs, would seem to have only a limited term of existence. They appear at one definite point of time, and disappear at another, although their coming and going is not abrupt but gradual. If these instinctive needs are not allowed to function during the course of their existence—if they do not work themselves through the individual's behaviour—they will disappear, never to return. James derived his law from general observation, but it has since been tested by experiment. Yerkes and Bloomfield observed the behaviour towards mice of kittens that had been fed with milk, and with meat and fish, for the most part cooked. In the course of the second month all eight of their kittens, coming from two different strains, showed the normal type of behaviour towards mice—the one strain earlier and the other later—quite like any ordinary cats, although these kittens had never seen a cat react to a mouse. The investigators conclude, therefore, that the instinct to kill mice appears usually at the end of the second month, though sometimes even a month earlier. This investigation is of special interest because, a few years before, another experiment upon the behaviour of cats was carried out in the same laboratory by Berry, who, among other things, dealt with this same problem. As a result of his experiments, Berry reached the conclusion that, although kittens have an instinctive

tendency to run after running things, they must nevertheless *learn* to kill mice, because their instincts do not carry them to this point. The apparent contradiction in these results is explained, however, by the fact that Berry's animals were already five months old when they first came in contact with mice. It would seem, therefore, that the instinctive disposition noted by Yerkes and Bloomfield in the second month had by the fifth month disappeared, which gives us a very pretty example of the transitoriness of instincts.[114] Similar exact observations in the case of man are lacking, and whether they are possible, in view of the greater complication of human behaviour, we can not yet say. McDougall, who limits transitoriness to a few special cases, does not find it an important characteristic of human instinct. He supports his view by reference to certain recent results in psycho-therapy, where pathological symptoms have been traced back to unwittingly suppressed, or improperly directed, instincts.[115]

It is not our intention to give a list of human instincts. Two-thirds of the first volume of Thorndike's comprehensive work are taken up with a consideration of man's original tendencies, and one may also refer to James for a discussion of this subject. For our part, we prefer to go into a few modes of behaviour appearing early in the course of human development, the instinctive character of which can hardly be doubted, since Köhler has also found them among the chimpanzees. I have in mind especially the instincts of cleanliness and adornment. We shall have to speak about walking as an instinct in the next chapter.

Regarding cleanliness, Köhler tells us he has observed but a single chimpanzee in captivity that was not coprophagous [a fæces-eater], and yet whenever an animal stepped into fæces his foot lost its firm hold just as a human being's would in a similar predicament; the animal then hobbled away until an

Köhler on Chimpanzee

opportunity was found to cleanse the foot, and in cleansing it the hand was never used, although but a moment ago the same substance was being conveyed to the mouth by the hand, the animal refusing to let go the substance even under severe punishment. In cleansing his foot, however, the ape must have a stick or a piece of paper or cloth, and his gestures showed unmistakably that the task was a disagreeable one. Indeed, there could be no doubt that the animal's behaviour was that of freeing himself from something nasty. This was also the case whenever any part of the body was dirtied. The filth was removed as quickly as possible, and so far as Köhler's observation extended, it was never removed by the naked hands, but always with the aid of something else, and by such methods as rubbing against a wall or upon the ground.[116]

Concerning adornment, Köhler found his animals prepossessed of a tendency to hang all kinds of things upon their bodies, after which "objects hanging about the body serve the function of *adornment* in the widest sense." Köhler believes, indeed, that primitive adornment does not depend upon its visual effect on others, but exclusively upon a curious heightening of the animal's own bodily feeling, pompousness, and self-consciousness, just as is the case with man when, for example, he drapes himself with a sash.[117]

It ought not to be difficult to observe similar tendencies in children; the existence of an instinct of adornment in particular might readily be determined with the aid of well-directed observations.[118] To make sure of inherited tendencies of cleanliness will doubtless prove more difficult, since education takes a powerful hold upon the child in this respect from the very beginning. It is possible, however, that paradoxes like those observed by Köhler in chimpanzees, though of a less disagreeable nature, might also appear in children.

§ 11—*Expressive Movements*

We turn now to a final group of infantile movements which occupy a unique position by virtue of the impression they make upon every one who has anything to do with children. These movements influence one's attitude toward the child, and give rise to the intimate relationship between the infant and the adults who attend him. Crying, laughing, and turning the head away, all of which have been previously mentioned, together with certain other responses we are about to describe, constitute what are commonly referred to as "expressive movements." These expressions occur spontaneously, without previous experience of them. Yet they seem to differ from other instincts, first of all, in that they do not stand in any direct relation to definite consequences. This distinction is not absolute, however, as we have seen in the case of turning the head described by Preyer, which was specified as being instinctive. One may say that crying continues until the child is relieved from a painful situation ; even so, the relation between this reaction and its consequence is a rather loose one, because crying is not of service to the child in the same way as sucking the mother's breast. In adults, most of the expressive movements seem to be entirely useless, and yet they still play an important part in their influence upon the behaviour of others. Thus Ordahl observed that when birds feed their young the largest portion always goes to those that cry most and loudest.[119] This social function is one of the most important aspects of expressive movements. Köhler describes the way in which chimpanzees almost invariably and immediately understand each other's expressions in states of excitement which involve the entire body.[120]

In calling these actions expressive movements, it is necessary to warn against a possible misunderstanding ; for although the movements to which we refer do ex-

press something, so that we are able to observe whether a man is pleased or angry; yet, as a rule, the man himself does not make these movements for the purpose of expressing anything at all. The idea that expressive movements are intentional—which in so crude a form would hardly be advanced by any one—is energetically opposed by Thorndike, who holds that the movements expressive of emotion may, on the one hand, be biologically more important and more original than the emotion itself, and that, on the other hand, while they tend to alter the situation for the agent, they do not serve as a means of communication. As a rule, the social effect of expressive movements will be quite direct. One may be led to comfort a child without first considering that he is unhappy, and when the mother bird gives the most food to the young one that cries the loudest, this also takes place without deliberation on the part of the parent.

The questions before us are two: How to understand social influences, and what the relation is between emotion and expressive movements. If we are content to fall back upon external inborn connections, which supposedly arise on account of their biological utility—as Thorndike does—we shall find no better explanation than we did in the case of instinctive activity. Furthermore, if "outer" and "inner" behaviour are anywhere actually related, surely the expressive movements must be instances *par excellence*. Can we believe that the emotions have obtained their expressive movements, or that these movements have obtained their emotions, merely by selection based upon fitness?

In a previous discussion of the general relation between sensory and motor processes (p. 113) we have found the latter to be a natural continuation of the former. If we define the concept of sensory processes somewhat more closely, we shall find that it is directly applicable to expressive movements.

Let us review the description Thorndike gives of the

behaviour of a cat confined in a puzzle-box (cf. p. 99). Many of the cat's movements may be called simply expressions of excitement. Thorndike explains them by his principle of "multiple response." The situation in which the animal finds itself is connected by innumerable nervous pathways with innumerable reactions—including, at least, all the different movements that can be observed. In course of time different nervous connections become successively functional, and occasion different movements. Instead of this explanation, we should say that from the diffuse though highly dynamic process of "excitement," which is the animal's immediate reaction to the situation of confinement, there issues an equally diffuse and dynamic response determined by the phenomenal phase of the situation—that is, by processes in the sensorium.

In all essentials the expressive movements are but simple instances of our general principle of behaviour. Köhler describes the process as follows: "If we were to represent behaviour graphically by means of a time-curve, the behaviour of fright might show an abrupt rise in the curve, followed by a gradual fall. The dynamics of the phenomenal or mental processes accompanying this behaviour would then be indicated by a curve of essentially the same character—and so will a purely electro-motor process in a photo-electric element when it is suddenly and briefly exposed to the light." Now, if the terms "abrupt rise" and "gradual fall," used in these three cases, be not merely analogous, but in some sense truly identical, "then, in principle at least, it is possible that an intrinsic relation exists between the mental processes of a living creature, and the total impression made by movement of the creature's limbs upon one who witnesses the movement." The connection between emotion and movement, including instinctive movement, is thus conceivable in a way which includes an understanding of the expressive movements.

In our first chapter it was noted that certain real entities of behaviour correspond to the total impression which an animal's behaviour makes upon us (pp. 21 f.). In addition to other characteristics, not less important in solving this problem, every form of behaviour has a certain articulation or phrasing. This articulation issues from a similar articulation of the central nervous processes of the acting individual. This central articulation in turn corresponds to the individual's "experience," which is articulated in a like manner. Thus the perception in the mind of an onlooker, if it be so constituted as to embrace what is going on in the agent, must itself possess a similar articulation. And hence the experience of the agent A, and of the observant B must resemble each other.

Köhler elucidates this point with a striking example. When a pianist moved by his feelings articulates a series of muscular innervations with varying degrees of phrasing, fixed time-relations are determined in the series of sound-waves, which constitute a sort of physical projection of the phrasing of his muscular innervations, thus conditioning in the mind of the listener an articulated auditory process which closely resembles the pianist's own nervous articulations.[121]

Bühler distinguishes in the first weeks of life four different expressive movements, namely: crying, smiling, head-deflection as a mark of avoidance, and pursing the lips. Concerning the first of these it may be remarked that to screaming, which is the sole type of crying at birth, weeping is added after the third week, at which time a true smile likewise appears, although even before this an expression can be observed which Preyer has called "contentment."

"The pursing of the lips, finally, is a peculiar gesture which can be aroused in the first weeks of life by touching the lips of a hungry infant with an object which is immediately withdrawn. The mouth at once takes on the peculiar shape characteristic of sucking. Later on

this pursing of the lips may be observed to accompany any kind of attentiveness." [122] The movement is directed towards a goal. The lips continue, as it were, to pursue the goal even after it has been withdrawn; in this respect the act is quite as instinctive as turning the head.

Lastly, facial grimaces occasioned by sours, bitters, and sweets, also belong to the characteristic movements expressive of emotion which appear at birth.

§ 12—*The Sensitivity of Infants*

We have now surveyed the movements made by a newborn infant. What, then, is the nature of his sensitivity? In other words, what sort of stimuli provokes his reactions, and how do the different senses share in the reception of these stimuli? We have formulated the question of sensitivity very cautiously, because there is no other way of testing the sensitivity of a newborn infant than by observing whether or not a controlled stimulation is followed by a reaction. In experimenting with an adult we can secure direct information whether a certain stimulus has affected him or not—whether, for instance, he has heard something or not. We can ask directly if a certain stimulus has been phenomenally apprehended, and thus limit the reaction to the observer's "inner behaviour." In the case of an infant, however, we are altogether dependent upon the evidence of his external behaviour. We must, therefore, be careful not to confuse the problem of consciousness with that of sensitivity.

From the first, all the sense-organs give rise to reflex-movements,[123] and hence sensitivity can be attributed to all departments of sense, yet the different senses exhibit great variation with respect both to delicacy and to the differentiation of their response to stimulation. Stern has clearly described these rela-

tions,[124] and we shall follow his division of the senses into three groups :

(i) *The Senses of the Skin*

1. Touch shows the greatest differentiation of response, since different reactions take place according to the particular point at which the stimulus is applied. This fact is apparent from numerous reactions with which every one is familiar. A touch in the region of the eyes occasions closing the lids ; a touch on the lips gives rise to movements of sucking ; contact with the palm of the hand causes the hand to close, and contact with the sole of the foot causes a spreading of the toes. But not all regions of the skin are sensitive in the same degree that they are in adults. According to Preyer, the mucous membranes of the lips and nose are hypersensitive in infancy, while the regions of the trunk, forearm and thighs, are hyposensitive.

2. The end-organs for temperature are to a considerable extent functional at birth. Bathing-water and milk must be of the right temperature or they are refused by the infant.

3. Sensitivity to pain, on the contrary, is subnormal. With reference to the senses of the skin it should be added that our knowledge of this subject is still superficial. According to the investigations of Sir Henry Head and his collaborators,[125] two distinct forms of sensitivity manifest themselves particularly in this field, the one being finely differentiated, localized, and graded (the " epicritical ") while the other is gross, diffuse, and of a kind described by physiologists as " all-or-none " (the " protopathic "). If this distinction were applied to the sensitivity of the newborn infant it might bring to light a series of facts. At any rate I am prepared to believe that the statement regarding pain should be revised by limiting it to epicritical sensitivity.

(ii) *The Chemical Senses and Sight*

1. Taste. Here again there are very distinct differences of reaction: sweet substances are swallowed; while those markedly sour, bitter, or salty, are rejected; at the same time we can readily observe facial expressions characteristic of sweets, sours, and bitters. Finer discriminations are not long delayed, as was evinced in the case of Preyer's son who refused thinned cow's-milk as early as the fourth day. The infant's preference for sweet things grows continually, so that he may even refuse the breast if the bottle-milk is sweeter.

2. Attraction- and avoidance-reactions can also be aroused by the sense of smell. Turning towards the mother's breast has already been mentioned; a positive avoidance of the breast can also be induced by smearing it with some evil-smelling substance.

3. We have previously discussed the important reactions of the eyes, including the pupillary reflex, the closing of the lids upon the incidence of a strong light, and the direction of the eyes toward bright objects. It should be realized, however, that there are enormous differences between the optical adjustments of adults and those of newborn infants. We shall have occasion to refer to a peculiarity in the visual sensitivity of infants when we come to consider the development of perception. This peculiarity has to do with a remarkable limitation of the field of vision with respect to its extension and depth.

(iii) *Audition*

Recent experimental investigators agree with previous observers that, with one exception, the first reactions of the infant to sounds and noises are of very diffuse character,[126] involving practically the whole body, and depending in their extent upon the intensity of the stimulus. The general character of these early responses

is muscular contraction, such as compression of the eyelids, wrinkling of the forehead, folding in of the lips, and so on. Löwenfeld interprets this mass-response as a reaction of flight, an attempt to get away from the stimulus, comparable to the closing of the lids upon the incidence of strong light, though less specific for the reason that the ear cannot protect itself against sound as the eye can against light. This interpretation seems plausible, although the one specific reaction which has been described is not easily reconciled with it. I refer to the turning of the head toward the source of sound which we have previously discussed.

On the other hand, as previous observations have shown, the infant can be quieted by sound-stimuli (so by whistling) as early as the first week of its life. The human voice seems to affect the child very soon after birth, and, indeed, the first differentiated reactions to auditory stimuli seem to be aroused in this way. The investigations of Hetzer and Tudor-Hart, and those of Mrs Bühler, confirm the previous observations, and at the same time deepen our knowledge of these reactions.

In Hetzer and Tudor-Hart's experiments responses were evoked by a number of different sounds, those of human speech being among the least intensive. Accordingly, the responses to speech were relatively rare during the first two weeks, but they increased rapidly within the third week. In the same week there also occurred the first *specific* reactions to the voice, while the reactions to other sounds remained as unspecific as before. The significance of these results will be discussed later.

When we survey these three main divisions of sensitivity (i-iii) we find that in general they constitute an ordered series of capacities. With the exception of sensitivity to pain, the skin-senses stand at the top of the list with reference to differentiation, while hearing is at the bottom, and the others lie between. However,

even during the first weeks after birth the development of the different senses is so complex that one sense may interchange its lead with another several times in this brief period. It would therefore be premature to insist on any general order of genetic development.

At the beginning of this section the manner in which we propose to consider the sensitivity of infants was defined, and thus far we have limited ourselves to objective behaviour. But now that we have answered the question of sensitivity under this limitation, we may proceed to consider the limitation itself, and to inquire if any features are to be found in the behaviour called forth by sensory stimulation which would warrant the assumption of a corresponding "inner" experience. In other words, we are now almost ready to take up the so-called question of consciousness, and shall do so in the last sections of this chapter. But, before that, another problem confronts us.

§ 13—*Dispositional Plasticity*

Up to the present we have been trying to learn something of the newborn infant's motility and sensitivity. In each inquiry we have had to do with inherited modes of behaviour. But we have not yet exhausted the description of the infant's endowments; for many unlearned reactions are not yet functional at birth, and do not attain their maturity until later. Even this leaves us with a considerable gap in our knowledge; because development is not alone a matter of maturation, but also one of learning. The modes of reaction that differentiate the adult from the newborn child are quite unlike those that differentiate a full-grown hen from a chick. The distinction between the development of a chick and the development of a human being is based upon individual acquisitions. Hen and man differ not only in their dispositions, leading to definite types of reaction, but first of all in

the fact that man acquires individual reactions of an incomparably higher type. And this capacity to learn is likewise an inherited disposition. In comparison with the lack of variability previously mentioned, the disposition to learn may be ascribed to plasticity, and a large measure of plasticity is one of the striking characteristics of man (cf. above Chapter II, p. 43). As Bühler remarks, plastic dispositions " do not completely determine what shall take place, since they are subject to modification by the activity itself." [127]

This conception of plasticity as dispositional may easily lead us into difficulties, if we think of dispositions only as certain predetermined bonds of connection in the system of neurones ; because from this point of view one is led to regard plasticity as nothing more than a lack of definite connections. Indeed, it has been argued that the fewer fixed connections an organism brings with it into the world, the less it is bound to employ definite reactions, and the more it can learn by experience. Thorndike seizes upon this explanation of learning, and traces its consequences with great thoroughness.[128] The fact that an organism possesses no definite bond leading from a situation S to a certain reaction R^1, can not be assumed to explain at all why the reaction should be R^2 or R^3 with which S is no more definitely connected than it is with R^1. The mere fact that my sneezing-reflex does not function would not of itself lead me to use my handkerchief, or to seek a physician who can remove some foreign body which has lodged in my nasal passages. Reactions such as these all demand a positive basis—*definite bonds*, as Thorndike conceives them—quite as much as does the sneezing itself. Neither can we say that a number of reactions which have no definite bonds with any particular situation are better suited to explain plasticity than just as many reactions, or even more, each of which is assumed to be definitely connected with a specific situation. Every connection must indeed

be a connection of some definite kind; consequently, indefiniteness can not furnish the explanation of plasticity. Plasticity, which Thorndike identifies with *multiple response to a single situation*, depends, according to his view, upon a fecundity of unlearned connections, leading from one another, until finally the end is attained.

Thorndike makes no distinction between rigid and plastic dispositions. To him all dispositions are either more simple or more complex bonds of connection between neurones. Consequently, for him the question of plasticity reduces itself to this: What kind of inherited bonds does man possess which other animals lack; and what bonds does man lack which make it possible for him to learn so much more than any other animal? [129]

Since we have refused to accept Thorndike's fundamental assumptions, the problem appears quite differently to us. Having found no reason for accepting a system of fixed bonds as the mechanism of *unlearned* functions, we are under no obligation to discover an apparatus of *learning* in any hypothetical system of bonds between neurones, whether inherited or acquired. A solution of this problem will be attempted in the next chapter, but so much may be said at once: If we abandon the view that learning is merely a matter of new combinations of connections already in existence, then plasticity becomes something more significant and more definite than even Bühler makes it out to be; because the question is now before us whether anything new can take place in the behaviour of an individual which can not be referred to a re-combination of old elements. If this question is answered in the affirmative, a line will be drawn between those organisms that are capable of creative responses, and those that are not; or at least a distinction can then be made with respect to higher and lower degrees of creative capacity.[130] It would then follow that plasticity must

be something more than memory—something more than the retention of an achievement by the simple means of effecting a new combination of reaction-pathways which the organism already possesses—and we could rightfully say that by virtue of his plasticity man is superior to all other living creatures. A further inference is also possible, and one which gives us an outlook upon the progress of our investigation. In § 9 we referred to two pairs of concepts which have become "the pivotal systematic categories of modern psychology": namely, stimulus and response, on the one hand, and original behaviour and habit, on the other. We can now see that the distinction usually drawn between instinct and habit no longer exhausts the possibilities of behaviour. Provision has been made for a new and important type of response which has no instinctive basis, nor has it yet become habitual.

In comparing Thorndike's explanation of behaviour with the one here developed, we find that the two methods of approach are based upon quite different principles. Thorndike confines himself exclusively to the question *where* the act takes place; and since for him all acts are alike, the problem reduces itself to the bonds established between separate neurones. We, on the contrary, find ourselves faced with the question *what* it is that takes place. And hence, we are not interested in a nervous pathway which always affords the same kind of excitation, but in the specific form of excitation requisite for the behaviour under consideration.

§ 14—*The Infant's Phenomenal Experience. Methodological Considerations with respect to the Question of Consciousness, and the Phenomena of Mental Configurations*

We come now to the last problem of this chapter. Thus far the infant has been described as an object of

external observation. We have noted what he does, and what kind of stimuli determine his responses; but the question remains, how does his behaviour appear to the infant himself? Does he know anything of his behaviour? Does he have any experience when he is stimulated and reacts? Is there any "descriptive" side to his behaviour? Or, to employ the usual terminology, is the infant "conscious" of his behaviour? The problem may be divided into two parts. Has the infant any consciousness at all, and, if so, how is this consciousness constituted in the beginning? The first question can be easily answered, and is of relatively little importance. Since the infant certainly attains consciousness after a shorter or a longer period of time, it is relatively unimportant whether consciousness begins earlier or later, and we have no absolute criterion by means of which a decision as to the time can be reached. It has often been thought that one must deny consciousness to the newborn infant upon the assumption that he is, at the time, a purely palæ-encephalic being. If this were so, the newborn child could not possibly have any experiences, but would live as a plant lives, without even hunger or satiety, pleasure or pain. But the behaviour of the brainless child reported by Edinger and Fischer indicated that, in comparison, normal children, even from the hour of their birth, differ from brainless children. The assumption that the cerebrum plays no part in early infancy is therefore unproved, and we are under no necessity of denying some form of consciousness even at birth. Against the hypothesis of an unconscious beginning, both the very early expressive movements as well as the "expression" upon the infant's features may be cited. Preyer [131] points out that even from the first day a contented facial expression can be differentiated from a discontented one, whereas Edinger and Fischer report that their anencephalic infant practically never showed the slightest trace of expression. We may therefore turn

to our second question, namely: What kind of experiences can the newborn infant have?

Since the infant's consciousness is not directly accessible to us, we can not directly conjure forth the world of a newborn infant in our own minds, any more than we can see with the infant's eyes, feel through his sense of touch, or be told anything by him. Therefore we must reconstruct his situation for ourselves. Why we should not altogether forgo this difficult task, has already been indicated (cf. pp. 15 f.); but how shall we begin it? The ordinary man, ignorant of psychology, assumes it to be self-evident that the world is essentially the same to every one; although to a newborn infant things must appear less complete, less distinct, and less familiar than they do to the adult. When such a person attributes a mental achievement to an infant—as, for instance, when he says that a child "thinks"—he really means that the child's thought is of the same order as his own, as though it were an imperfect copy of what goes on in his own mind. A person somewhat better acquainted with psychology would perhaps sneer at this naïve conception, but it may be doubted if the position he would take is necessarily a better one; for what he usually does is merely to apply to the suckling a theory derived from current psychology, which enables him to define mental "incompleteness" by attributing to the infant fewer sensations, no associations, etc. It need scarcely be repeated that a true psychology of childhood can not be achieved in this way. Indeed, we must begin with the "specific beginning-stage" of development whose peculiar nature it is our task to discover.

To those for whom psychological ways of thinking are unfamiliar, the following example may clarify the problem. Although two men are born into the same real world, its phenomenal aspects are not necessarily the same for each. It is often said that there is no use in disputing about matters of taste, since of two

persons in the very same situation one may find himself altogether dissatisfied, while the other is charmed. It is the task of the psychologist to trace this difference in reaction to its source. This done, it will often be found that, quite apart from feeling and evaluating, persons of conflicting tastes have actually experienced quite different phenomena. For example, in looking at a picture one person sees nothing but a confusion of clashing colours, while another sees an admirable and expressive work of art; or, again, where one person hears only a chaos of clangs, another is being impressed by a richly ornamental musical theme. The examples chosen are as obvious as possible, in order to make it quite clear to the reader that the same external situation may furnish phenomenal contents which are entirely different. In each example it can be said that the experience of the first person was less adequate than that of the second; but it is obviously impossible to believe that this inadequacy rests upon fewer sensations or upon fewer associated ideas. Let us now compare these examples with the infant's consciousness. In asking how the world is reflected in the experience of a newborn infant, the fact that the world is reflected very differently, even in the phenomena of adult experience, permits us to make use of just such differences as our examples have furnished in arriving at a correct description of the "inadequacy" of an infant's mind.

The argument, which has previously been negative in trend, now becomes positive. The objective world does not suffice to determine the experience of an individual; to this must be added the constitution of the individual himself. The newborn infant *experiences* the world *differently* from us adults, just as an unmusical person *hears* a symphony *differently* from one who is musical.

But how can we find out the nature of this difference? How shall we proceed to reconstruct the phenomenal

world of a newborn baby? Our previous considerations indicate that our reconstruction of the infant's consciousness must "fit" the observed events and conduct which constitute its "objective" behaviour. That is to say, the infant's consciousness and behaviour must fit together in the same way in which the phenomenal world of the adult fits his objective behaviour; for, as previously noted, all overt behaviour, every movement, every act, is the subsequent phase of a comprehensive total process originating in the sensorium, using this term in its widest sense.[132]

It ought, therefore, to be possible to turn the results of experimental psychology to account in the solution of our problem, without falling into any of the errors against which we have warned. If we find the behaviour of the infant to be but slightly differentiated, as compared with our own, then we must try to find some movements in our own behaviour which are also slightly differentiated as compared with other movements. This done, we can compare the two with reference to the phenomena usually connected with more and with less differentiated movements, and if any characteristic distinction is found, we must then try to carry it over to the phenomena of infancy. In a concrete case we shall have to examine each bit of infantile behaviour for itself, and work out its typical differences from the corresponding behaviour of adults, before we can proceed to reconstruct its phenomenal aspect. We must deny that "objective" and "subjective" behaviour have no inner connection, or are simply bound to one another like the obverse and reverse of a coin which might be stamped in any way; for, if this were so, we might as well give up at the outset any attempt to reconstruct the infant's experience. But, on the contrary, we insist that behaviour can not be described in its entirety until we are acquainted with both its aspects, and that only then can we give it an adequate explanation. The position we

have taken holds, not only for infancy, but for the whole of child-psychology, in so far as it is concerned with the phenomena of mind. An older child is not a "little man"; and just as his behaviour differs from that of an adult, so also do his experiences differ.

How, then, shall we proceed to reconstruct the experiences of a human being during the first days of life? First let us inquire, what are the most important features of his behaviour? Quite obviously, they are his gross bodily conditions, such as hunger, satiety, fatigue, freshness—all of which can be understood in a purely objective way. Let us consider these conditions as they seem to us. When we "feel fresh," there are no very definite reactions connected with this condition (as there are, for instance, when we drive a nail into the wall). Our feeling of freshness can be expressed in all sorts of movements, so long as we move at all. The situation is reversed, but not more specific, when we feel tired, and seek quiet. Even when we are hungry, all that this phenomenon determines is that we should do something in order to obtain food. Whether we cut a piece of bread, seek a restaurant, or do something else, depends upon a thousand things which have nothing to do with the feeling of hunger. And when we are satisfied, we simply stop eating. In all these instances the objective behaviour of the infant appears to be essentially the same as our own. When he is refreshed, he moves about; when he is tired, he lies still; when he needs nourishment, he cries until he is taken to the breast; and when he has had enough, he stops sucking. To be sure, his behaviour is very slightly differentiated, but so would ours be under similar circumstances. Nevertheless, his behaviour is of enormous biological significance, so that we may quite justly conclude that the states we recognize as hunger, etc., are among the first experiences an infant has; and that they are, in point of fact, phenomenally quite similar to our own.

But what can be said of those experiences which put us in touch with the outer world? How are the perceptions of an infant constituted? We find the new-born child capable of movement whenever external stimuli come in contact with his senses; that is, whenever the equilibrium of his condition is disturbed. For instance, a bright object appears in the field of vision, and the eyes move; a contact is made with a certain place on the hand, and the fingers close, etc. In every case a state of rest is interrupted; into the already existing world wherein the child was at rest a new factor has been introduced which disturbs his quiescence. If we wish to reconstruct the phenomenal counterpart of this objective behaviour we must consider the child's state as a *whole*. Consequently, we ought not to say that the child sees a luminous point; but rather that the child sees a *luminous point upon a relatively indifferent background*; or, in the case of touch, that pressure is felt upon the hand, which before had been lacking in phenomenal distinction. Generally stated, *from an unlimited and ill-defined background there has arisen a limited and somewhat definite phenomenon, a quality*. Whether or not the background existed phenomenally even before the new factor emerged from it, will be discussed later. Here it is sufficient to note that, when a quality appears, the "indifferent" ground must also be considered as more or less "uniform." We are presupposing that before the appearance of the stimulus the child was at rest, and not moving. If we infer phenomena of experience from behaviour, an undifferentiated phenomenon must correspond to the absolutely undifferentiated behaviour of quiescence. The reader should not forget that we are speaking of the earliest beginnings of consciousness; and that it is the very first experience of the child that we are attempting to characterize. Our characterization then, is this, that the first phenomena are *qualities, or figures, upon a ground*. Introducing at this point a new con-

cept, we add that they are the simplest of *mental configurations*. The phenomenal appearance in consciousness divides itself into a given quality, and a ground upon which the quality appears—a level from which it emerges. It is, however, a part of the nature of a quality that it should lie upon a ground, or, as we may also say, that it should rise above a level. Such a co-existence of phenomena, in which each member "carries every other," [133] and in which each member possesses its peculiarity only by virtue of, and in connection with, all the others, we shall henceforth call a *configuration* (*Gestalt*). According to this view, the most primitive phenomena are configurative. As examples, we shall take the luminous point set off from a uniform background; something cold at a place upon the skin set off from the usual temperature of the rest of the skin; the too cold or too warm milk in contrast with the temperature level of the mouth-cavity. We attribute configurations, also, to such reactions as the rejection of milk when it is not of the right temperature; thus milk in the mouth may lead either to an "adequate" or to an "inadequate" configuration.

To many this view of the constitution of the most primitive phenomena will appear very odd indeed; for it assumes that a certain order dominates experience from the beginning, whereas we should be in much better agreement with current views if we were to assume that order comes only as a result of experience—a theory which has given rise to the view that the consciousness of the newborn infant is nothing but a confused mass of separate *sensations*, some of which are present earlier than others, because of the earlier maturation of their appropriate brain-centres. Upon the basis of such a theory, the sense of vision would seem to supply the child with a chaotic mass of achromatic and chromatic impressions, like the colours upon a painter's palette, from which experience would proceed to choose the ones that are requisite to constitute

his perceptual world.) And this doctrine is founded upon one of the fundamental presuppositions with which psychology has long worked; namely, that single mental units called sensations are aroused in a simple manner by stimulation, and from them every other kind of experience is derived by a process of association.[134] The behaviour of the child, however, certainly does not of itself suggest any such presumption. And a few arguments may be added that directly contradict it, and at the same time support our hypothesis of the configurative character of the first sensory phenomena.

1. Our principle of reconstructing the phenomena of infantile experience in such a way that they will fit the child's behaviour, would certainly not lead us to assume that a newborn infant possesses an abundance of mental phenomena. On the contrary, (his behaviour seems to demonstrate that there are very few motives which can set him in action.[135])

2. If the theory of original chaos were correct, one would expect "simple" stimuli to be the first to arouse the reaction and interest of the child; because simple stimuli ought to be the ones first to be singled out from the chaos for association with one another. But all our experience runs counter to this assumption. It is not the stimuli the psychologist takes to be simple, because they correspond to his elementary sensations, that are most influential in the behaviour of a baby. The first differentiated reactions to sound are aroused by the human voice whose stimuli (and "sensations") are very complicated, indeed. For instance, at the end of the first month the infant begins to scream when it hears another baby scream. Between the first and second month the infant reacts to the human voice with a smile, at first without differentiating between a friendly, neutral, or scolding voice. This differentiation occurs in the fourth or fifth month, when a smile is

the reaction to friendly and inviting speech, while angry words evoke crying and general symptoms of discomfort. Nor is the interest of a suckling aroused by a single colour, but by human faces, as Miss Shinn has expressly reported to be the case with her niece after the child's twenty-fifth day. During the second month the friendly gaze of an adult makes the infant smile. At the end of two months, the mere presence of an adult may have a quieting influence upon the child. This reaction is so specific that during the first half-year of his life the infant will smile only at another human being—not, for instance, at a toy, however much the toy may attract his attention. Think what sort of experience must parallel the process of distinguishing, among an infinite variety of chaotic images, the father's from the mother's face (and more than this, a friendly from an unfriendly countenance), the sensations of which are constantly undergoing change. On the other hand, "even in the second month it can sometimes be noticed that the child does not remain indifferent to certain frequent impressions—especially its mother's face and voice—but greets them with a faint smile. After three months this cognition has advanced to differentiation, and the child's attitude is quite different to persons it knows and to strangers."[136]

As early as the middle of the first year of life an influence of the parents' facial expression upon the child may be noted. According to the chaos-theory, the phenomena corresponding to a human face can be nothing but a confused mass of the most varied light-, dark-, and colour-sensations, all in a constant state of alteration—changing with every movement of the person observed, or of the child himself, and likewise subject to every change of illumination. Yet the child recognizes its mother's face as early as the second month, and in the middle of the first year the reaction to a "friendly" face is quite different from the reaction to an "angry" face. Furthermore, this difference is of a

kind which obliges us to conclude that "friendly" and "angry" faces are phenomenal facts to the infant, and not mere distributions of light and shade. It seems quite impossible to explain this behaviour by experience, upon the assumption that these phenomena arise from an original chaos of sensations in which single visual sensations combine with one another, together with pleasant or unpleasant consequences. One of Köhler's observations is here in point [137]: "By suddenly showing signs of the greatest terror, while staring at a certain spot as though possessed, it is not difficult to make all the chimpanzees in the station look at the same place at once. Immediately all the black company starts as if it had been struck by lightning, and proceeds to stare at the same spot, even though nothing is to be seen there. According to the usual view this involves an inference drawn by analogy from what is taking place in 'my consciousness.'" The animals understand this terror-stricken direction of the gaze *immediately*, and an inference by analogy from Köhler's consciousness of terror would be an altogether absurd explanation.

Is it not possible that phenomena, such as "friendliness" and "unfriendliness," are very primitive—even more so than the visual impression of a "blue spot"? However absurd this possibility may seem to a psychologist who regards all consciousness as being ultimately made up of elements, it ceases to be absurd if we bear in mind that all psychological phenomena stand in the closest relation to objective behaviour. "Friendliness" and "unfriendliness" certainly influence behaviour, whereas it is not easy to understand how the behaviour of so primitive an organism as the human infant could be motivated by a "blue spot." If phenomena are to be construed from behaviour, must we not attribute to them, first of all, such properties as might occasion activity? Certainly the fact that something is being done furnishes the basis for an inference that certain phenomena accompany the behaviour in question. But

this means that we must assume that features like "threatening" or "tempting" are more primitive and more elementary contents of perception than those we learn of as "elements" in the text-books of psychology.

This view is obviously in flagrant contradiction to empiricism, according to which all these emotional or dynamic properties are acquired or conditioned. But it would be wrong to call it aprioristic, as Piaget and Rignano do. Piaget defines what he calls psychological apriorism in the following way: The organism assimilates the action of the environment according to its own structure which is independent of the environment and resists any modification by outside forces.[138] Our view is incompatible with such an apriorism, for the simple reason that this apriorism is as little in harmony with the facts as traditional empiricism. Our explanation of how we understand expressive movements (in § 11 of this chapter) ought to have made this clear. Instead of introducing the assumption of an apriori "knowledge of general you-ness" (Driesch [139]), we have shown that the experience of understanding is a result of *specific* outside forces impinging upon the organism. (Although the organism is also specific, dominance was given to the specificity of the outside forces.) Accordingly, much is understood without previous experience. In other words, the organism will react in ways appropriate and adequate to a large variety of different situations. It could not do this if it were simply to "assimilate the action of the environment according to its own structure"; because such an apriorism would never explain why the reactions were adequate to the situation. Of course the organism must also fulfil certain conditions; the greatest violinist cannot produce the same effect on a cheap instrument that he can on a Stradivarius or an Amati. But no one would distinguish between the contributions made, on the one hand by the violinist and on the other by the violin, to this performance. In this connection I may recall the

discussion of the concept of inheritance in § 9 of this chapter which contains a similar argument.

According to my view each reaction consists of a distribution of forces in the organism. This distribution of forces is not different from any other physical distribution of forces, and is determined by the structure of the organism and the distribution of forces on its surface. Each reaction is a function of many variables, which, as we have previously seen, does not mean a sum-total of different processes. Thus the dilemma of empiricism *vs.* apriorism (nativism) disappears.

We have previously remarked that the ground of a figure-ground structure is relatively "indifferent." We must now add that a figure is also something positively or negatively "interesting." For similar reasons Scheler concludes "that of all the external objects apprehended by man, 'expression' is the very first." [140] With this statement we are in full accord, if the connotation of the term "expression" is made sufficiently broad to include such behaviour as a response to light in darkness.

Referring again to what has already been said about the perception of "expression" (pp. 129 f.), we need only add one further remark upon this subject. If we accept phenomena such as "friendliness" and "unfriendliness" as primitive, we must maintain that primitive phenomena are indivisible into perceptive and affective elements, and that a "subjective" feeling does not exist alongside of, though apart from, "objective" perceptions, but that *qua* phenomenon, the primitive world of experience embraces affective determinations just as it does those we are accustomed to characterize as objective. Thus we find ourselves again in complete accord with many standard authors.[141] Folk-psychology teaches the same thing—namely, that for men of primitive culture the world is full of qualities which we are accustomed to characterize as emotional, and which we consider purely subjective, egotistical, ingredients.[142] Of course, what we imply is that the first

perceptive phenomena already carry with them the characteristics of objectivity, which phenomena such as freshness and hunger lack. It goes without saying that one must not use the term "objectivity" in the sense in which it is employed by the philosophers. All we mean is that perceptive phenomena are something other than "organic feelings"; and that the distinction between subject and object is not learned, but is given, no matter in how primitive a form, in the very first phenomena of the infant mind.

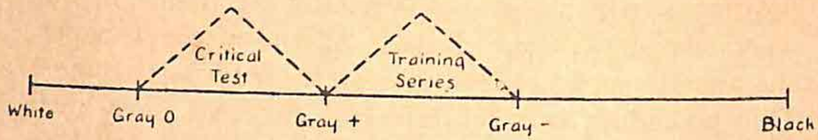
3. Brod and Weltsch [143] advance the following argument in opposition to the view that mind is originally a mosaic of innumerable sensations. It happens sometimes, either intentionally or through inattention and fatigue, that the developed phenomena of adult life are "screwed down" in the direction of a less developed state. We have all experienced states of distraction in which our consciousness is transformed into an inarticulate unity. The world then appears, not variegated, but monotonous. The assumption of an original multiplicity would be untenable in the light of this experience; because we have here the inarticulate uniformity already described as the phenomenal ground from which a *quality* emerges. Imagine this modification, which our adult world of perception sometimes undergoes, carried to an extreme. May we not assume that we would then revert to the first and most primitive of conscious phenomena? The only question would be where to set a limit, for ultimately this limit appears to lead us to nought. In the end, with absolute monotony, have we any consciousness at all? Previously we left open the question whether the inarticulate ground-work upon which the quality of an experience appears is already there before the quality emerges, or whether it arises with the quality itself. If the ground can have no existence apart from the quality or figure which emerges from it, then the most primitive pheno-

menon of consciousness is not the inarticulate groundwork, but the figure-ground structure. This opinion seems to me the more tenable of the two, because the phenomenon of a uniform ground-work, involving no disturbance of equilibrium, would be meaningless for behaviour. Furthermore, this opinion is directly supported by certain marked disturbances of perception due to organic brain-lesions; it having been found that certain patients are unable to *see* a complicated figure *at all* when their condition precludes the possibility of correspondingly *complex* psycho-physical processes.[144]

Ground and figure, since they are phenomenally inseparable (cf. above), must arise together. A part of the world is thus differentiated, and appears as a quality, whereas whatever remains may still appear as a uniform ground, though in reality it is extremely complex. I emphasize this statement in order to give point to the following fact: we can not construe the phenomenon corresponding to a given stimulus-pattern as though each particular stimulus had its own special phenomenon, such as can be discovered under the conditions of a psychological experiment which analyzes the stimulus-pattern into discrete stimuli, and studies their phenomenal correlates separately. Indeed, the assumption commonly made, that sensation is determined once and for all by its stimulus, will simply have to be abandoned.

4. There is direct proof that simple configurations must be regarded as very primitive phenomena. It is customary in animal psychology to perform the following type of experiment, known as "selective training." An animal is presented with two stimuli, such, for instance, as a lighter and a darker gray paper, and is trained to seek food with reference to one of them, but not with the other. It has been thought that in this way one could test two things, first, whether the animal experiences two phenomena, or sensations, cor-

responding to the two stimuli, and, secondly, how his memory operates with and after training. Leaving aside the second problem, let us consider the first one. It is usual to explain training in the following way: The animal learns to seek one sensation, and to avoid the other. Each sensation, therefore, becomes connected with a different mode of behaviour. We may call the sensation the animal seeks the "positive," and the other the "negative," and may apply these terms to their corresponding stimuli. Köhler undertook the following experiment. He first trained an animal to choose the brighter of two grays. After the training had been brought to a successful issue, "critical tests" were made in which two gray papers were again presented to the animal, so chosen, however, that the previously employed brighter and positive stimulus was retained, while the darker and negative one was replaced with a paper still brighter than the positive stimulus of the training-series. There had been no training with the new paper; it was therefore neither positive nor negative. In Figure 5, from which one



[*After Köhler.*

FIG. 5.

can comprehend the entire scope of the experiment, it is indicated as "Gray 0." What will the animal do? The new gray is neither positive nor negative, but neutral, while beside it lies a gray strongly positive as a result of many repeated experiments. If the theory of specific response to specific stimuli is correct, we should expect the positive stimulus to be selected in a majority of cases; there certainly would be no reason to suppose that the neutral gray would be more frequently chosen.

The experiment can be varied by making the darker

gray the positive colour of the training-series, and then using a still darker colour instead of a brighter one in the critical tests. Or, again, one may retain the negative rather than the positive stimulus in the critical tests, and associate with it a gray which is still farther removed from the positive colour than it is from the negative, though in the same direction. In the interest of brevity, we shall confine ourselves to the first case.

Köhler carried out a long series of careful experiments of all kinds with hens, with chimpanzees, and with a child nearly three years old. In order to give the reader an idea how these experiments were performed, I will briefly describe the tests with hens. A hen was placed in a cage, one of the sides of which was so wired that the fowl could easily thrust her head and neck outside. Before this side of the cage a horizontal board was placed from which the hen could eat. Upon this board, adjacent to one another, the two papers were laid which were to be employed in training, and upon each paper an equal number of kernels of grain were placed. If the hen pecked at the grains upon the positive paper, she was allowed to eat them all, but whenever she pecked at those on the negative paper, she was shooed away, and thus prevented from eating.[145] This procedure was continued on different days until the hen no longer attempted to peck at the negative paper. The position of the papers was frequently altered so that the positive stimulus lay now at the right, now at the left, in order that the fowl should not learn always to peck in the same positional direction. In order to complete the necessary training, from four hundred to six hundred trials, and more, were requisite. When this training had been achieved, Köhler proceeded with the *critical tests* in which the fowl was allowed to eat without hindrance all the grains from whichever paper she might choose. The experiment was then at an end and could be repeated.

The results of these experiments on hens contradicted

altogether the expectations based upon the sensation-theory. Among four hens, two of which had been trained to select the brighter, and two the darker gray, the newly introduced neutral paper was selected fifty-nine times out of eighty-five critical tests, whereas the original positive paper was selected only twenty-six times. On the basis of the sensation-theory, the opposite was to be expected; at least, the positive colour should have been chosen not less often than the neutral. The presuppositions of this theory must therefore be false.

How, then, can we explain the outcome of these experiments? What can have remained over in the critical tests from the situation of the training-series except the objective presence of the positive stimulus? "In this special arrangement, where two different colours are placed side by side in an otherwise symmetrical figure of a very simple form, introspection shows that what is characteristic of the experience is not the mere presence of one colour lying by itself, and another colour lying by itself, but the 'togetherness' of the two colours."[146] Obviously this dark-bright pattern, this colour-figure, is retained when one passes from the setting of the training-series to that of the critical tests. It can therefore be inferred that, in the majority of cases, the choice was determined by this pattern, rather than by a retention of the absolute positive quality of the training-series. If the behaviour of the fowls depends primarily upon the characteristics of a configuration, rather than upon the absolute constitution of the colours employed, the conclusion is justified that the phenomena involved in these experiments are configural. Furthermore, the fact that these experiments were carried out with hens, proves that such configurations are possible, not only in a developed state of intelligence, but also in a very primitive type of mind.

In the experiments Köhler performed with a child,

two boxes were placed before the child, one with a brighter and the other with a darker cover. The child was told to take one, and without further aid he soon learned to choose always the brighter box which contained candy, and to reject the other box which was empty. After two days (forty-five trials), when the child was able to make his choice virtually without error, the critical test was given; the result of which was the same as with the hens, though far more decisive. With reference to comparative brightness, and the rejection of the "absolute" colour, the child invariably and without hesitation chose the new and brighter box.[147]

We have noted that "absolute" choice sometimes occurred with the hens. In a special series of tests, Köhler modified the conditions in order to find out which were favourable to the operation of the "absolute" and which to that of the configural factor. His results indicate that the operation of the "absolute" factor ceases to be effective with time, and is quickly forgotten. "The truly essential, lasting, and definite product of learning," he tells us, "is dependent upon a configurative function."[148] This statement holds true for all the more primitive forms of life in a measure in which it does not hold for adult human beings. An adult's choice would not have been unhesitatingly in accordance with the structure of the pair, as was the child's. There would have been a question in the adult's mind whether to behave with reference to this configuration, or with reference to the absolute grayness already known to him. Only when we adults are called upon to judge of colours that are qualitatively very like one another—that is, when a small enough interval has been chosen between the negative, positive, and neutral colours—do we likewise fall under the compulsion of a configurative choice. I have frequently verified this result in class-room experiments. Indeed, the difference between the behaviour of adult and child

shows clearly that the "absolute" factor is not more primitive, but instead is a product of higher development. In similar experiments with children, Riekel found that the number of responses to the "absolute" gray increased with increasing age.[149] It follows that these "absolute" factors can not be identical with the "simple sensations" which, according to the older view, lie at the foundation of all training.[150]

As a consequence of this observed difference, we may conclude that simple configurations are primitive modes of behaviour which in no wise presuppose the existence of absolute sensations. Our presumption that the very first phenomena of the infant mind are qualities of this figural sort is likewise supported by these results.

5. A final argument in support of the configurative nature of primitive phenomena can be based upon the results of Katz's investigation of our sense of touch.[151] From a large number of diverse facts Katz reaches the conclusion that moving forms precede static forms in perception. He also points out that the attention of a young child is more readily caught by objects moving with a moderate velocity, than by objects at rest. A large amount of experimental work, both extensive and intensive, has been carried out since the year 1912,[152] the results of which all lead to the conclusion that the perception of movement is a configurative process. Thus configurations are characteristic of primitive mentality, and not the impression of and response to discrete stimuli.

Again it should be emphasized that the configuration, which we have assumed to be the first phenomenon of mind, must be thought of as very simple indeed—merely as a quality emerging from a uniform ground. Accordingly, we must not think of these phenomena as being at all like the experiences we adults have; at the beginning, only the slightest degree of complexity

and definiteness can be ascribed to them. But in the following pages we shall become acquainted with configurations as they appear at a higher level, which will enable us to study their development.

Finally, the expressive movements already described can be connected with this primitive figure-ground structure. Crying and whining, which greatly vary in their expression, can be related to the phenomena of both ground and figure; the first in states like fatigue, hunger, and rest, the second with reference to unacceptable objects and localized pains. The early expression of contentment is obviously a pure ground-reaction, while the later-appearing smile is occasioned by a figure. Turning the head, pursing the lips, as well as eye-movements of fixation, can all be regarded as figure-reactions.

Chapter IV

SPECIAL FEATURES OF MENTAL GROWTH

A General Statement of the Problem. How New Types of Behaviour are Learned

§ 1—*Four Ways in which the Mind Grows*

We now know how the newborn infant begins his journey through life, and how he is equipped to undertake the immense task of becoming an adult and entering the circle of human society as an independent member. Let us therefore accompany him on his way, in order that we may observe his growth and development, and at the same time learn something of the laws in accordance with which growth and development take place. The principles will again occupy the foreground of our attention; because for our purpose the problem of development itself is of greater importance than the detailed facts of behaviour. Accordingly, our attempt will be to point out the nature of man's achievement in the course of his growth.

With this end in view, the first questions to be asked are: (What the infant has to acquire, and in what directions his behaviour must develop.) To these questions we can answer that it is possible to differentiate roughly four different ways in which the mind grows.

1. The first is concerned with purely motor-phenomena. (Movements and postures which appear at the beginning of life must be carried out with greater completeness; new movements must be built up and

made more or less perfect. Beginning with the activities of grasping and locomotion, one attains in due course the ability to speak, to write, and to perform musically, gymnastically, in sport, in play, etc.

2. The second way of development lies in the field of sensory experience. Here the magnitude of the task is even more obvious. We have already tried to show how simple must be the first perceptual phenomena of the human being; although this simplicity is, to be sure, of a quite different sort from that of the so-called "simple" sensations. Out of these rudimentary phenomena of dawning intelligence, our richly furnished, multi-coloured, and finely organized outlook upon the world must evolve. We have seen that, amid a multiplicity of things supplied by the environment which might be operative upon the child, only a very few are at any time effective. In the course of his development, however, this multiplicity must be mastered. The requirements which the adult's life brings to bear upon his behaviour are so numerous that they can in no wise be satisfied by the primitive phenomenal configurations of infancy. Gradually, therefore, the phenomena of the child's mind must be adapted to the innumerable stimuli which arouse them. The nature of this task can be made clear by an example. Consider the processes involved in deciphering a puzzle-picture, where, out of a confusion of irrelevant lines, the figure of a cat suddenly springs forth. Think now of a puzzle-picture, constructed not merely to show a cat or some other figure, but consisting in a chaos of lines and surfaces, which, however, either suddenly, or by successive stages, make possible the recognition of a landscape or a group of human beings. This example is related to the subject under discussion at the close of the last chapter (p. 142), where we were concerned with pointing out differences in the phenomenal world as they appear to different human beings who never-

theless observe the same actual world. The example is therefore chosen in order to indicate the problem which confronts man in the development of his sensory capacities. Briefly stated, the primitive, disjointed phenomenal patterns of infancy must be replaced by an integrated, membered, and effectively composed outlook upon the world.

3. But external and internal behaviour are not two opposed and isolated systems; for in truth the problem of behaviour is to carry out appropriate actions which involve the motorium in situations that are mediated to the individual through his sensorium. Along with purely motor and purely sensory acquisitions, we must, therefore, place those which are at once sensory and motor; meaning thereby the co-ordination of explicit with implicit forms of behaviour, and those adjustments of movements to phenomena without which an individual could never lead an independent life. To give a very elementary example of this, we may recall the saying that a burnt child fears the fire. Here the co-ordination of an avoidance-reaction with the phenomenon of fire is an acquisition that takes place after the original act of grasping has led to the painful experience of being burned. In this same connection we may recall the modification of instinct observed in the case of Preyer's boy, who preferred a bottle of sweet milk to the breast.

Having emphasized the close connection between the sensorium and the motorium we must now point out that in reality all purely motor acquisitions, classified under the first heading, contain a sensory component. In such activities as those of speaking and writing, this component is quite obvious. Deaf persons learn to speak imperfectly at best. The same thing is true, however, in forms of behaviour which require special motor practice, as, for instance, playing tennis; for here, too, it is not merely a matter of repeating the

same stroke, but of administering the right kind of a stroke whenever and wherever the ball is met.

Even in many quite early activities a sensory component enters, as can be understood by reference to walking, which is not a stereotyped movement. Not only does the tempo of walking greatly vary according to the occasion for locomotion, but, in addition, walking-movements are directed in accordance with the characteristics of the ground, being adapted to its irregularities without our cognizance of them. The process is more or less automatic; that is to say, the brain-centres which regulate walking must receive reports from the outer world regarding the nature of the ground passed over, and these sensory impulses regulate the movements made, though they need not lead to consciousness. To employ a striking example, consider how differently one walks when one has a sore foot, and how impossible it is under these circumstances to place one's feet normally even with the best of will. The nature of the connection between sensorium and motorium becomes still more evident when we consider another type of movement described in detail in the preceding chapter. If we chance to be gazing into the distance when suddenly there appears a striking object near at hand, this object will be fixated, and the eyes will accommodate to it. The reaction, especially the accommodation, is quite involuntary, and the sensory impulse by which it was started occasions a phenomenon in consciousness only after the movement has taken place, and the eyes have been directed upon the new object. The point of view from which we have found it desirable to consider this connection between sensory and motor behaviour, is that of regarding the whole procedure as an interconnected system in which the motor and the sensory processes are not independent, as they would be were they connected by external bonds. We shall retain this conception here; for even acquisitions of a purely "motor"

type presuppose an integrated, sensori-motor process. Every movement occasions a new sensory impulse in the brain-centres, which in turn contributes to the motor process. A proof of the important contribution made by sensory impulses to motor processes is furnished in the disturbances of walking that occur with locomotor ataxia (*tabes dorsalis*). In this disease it is not the motor but the sensory centres that are attacked, and yet a complete paralysis results. The patient, however, may learn to walk again if he can learn to make use of other sensory impulses than those of the tactual field which are lost. For instance, optical impulses may be employed, but the patient must then learn to regulate his walking by his eyes; that is, he must constantly watch his feet. Since in this manner a very considerable improvement in his performance is possible, it appears that the disturbance does not involve the motor centres; but it is also evident that some sensory impulse is necessary for each movement. The same conclusion has been reached from the physiological investigation of animals in which certain sensory centres have been destroyed.

The converse of this proposition is also true; for a "purely sensory" knowledge of the world as described under our second heading also occurs in co-operation with movement. Indeed, articulation and differentiation in perception are greatly facilitated by movement of any kind. Think, for instance, of grasping and touching, and also of "the line of regard" in vision, and of the movements of the head involved in spatial orientation. In his study of the "world of touch," Katz concludes that in primitive stages of development movement is a necessary condition of perception, and that the "touch-world" of adult human beings is still very close to the primitive level.[153]

We have tried to show that, strictly speaking, their are no "purely motor" or "purely sensory" acquisitions, and yet it is quite justifiable to distinguish the

sensori-motor group from the other two. The object of the sensory and motor groups, taken separately, is to determine the acquisition, either of an external motor, or of an internal sensory mode of behaviour; whereas the problem of the sensori-motor group includes the correlation of these two. This third type of development has therefore to do with uniting phenomena and movements, either of which can exist apart from the other, in one total form of behaviour; for instance, a hen can run, and it can see black and yellow striped caterpillars, but the tendency to run away *when* it sees these caterpillars is acquired.

4. From the third type of development we pass directly to the fourth. When we are suddenly confronted with the problem of adjusting ourselves to a new situation, we do not as a rule respond at once with an appropriate form of behaviour—rather, the reaction is checked while we consider the matter; that is to say, between the stimulating situation and the behaviour of reaction there occur certain phenomena which do not need to correspond directly with anything actually or objectively present. The following is a simple example. A child, finding itself alone, sees before him a tempting dish of sweets; then it occurs to him that he has been forbidden to take sweets without permission; accordingly he hesitates as to what he shall do. Should he leave the dish untouched, his behaviour with respect to the stimulating situation would be determined by the phenomena which have intervened. In the course of development interventions of this sort play a constantly increasing part. Whereas originally the reaction follows directly upon the stimulus, intervening members become more and more numerous, and more and more important, as development progresses. Our most significant accomplishments rest upon their employment, and their acquisition is therefore an essential task of development. By means of

these intervening members, we are able to disengage ourselves more and more from our immediate surroundings, and it is in this way that we are able to control nature to the degree that we do. Education finds one of its chief tasks in promoting this kind of development; for the best of what we learn at school is not the sum of positive knowledge acquired, but that we learn how to think, so that we can assume an independence which rests upon our ability to supplement the situations that confront us with appropriate intervening phenomena.

We have previously selected our examples from what one pleases to call the intellectual domain, but ethical conduct belongs in the same category, and behaviour must also develop ethically, so that it need not depend upon environmental conditions alone.

We shall call this general field of behaviour *ideational behaviour*. Here again the definition is not actually so sharp as the classification suggests; for the ideational field depends most intimately upon the sensory, and any means that enable us to become independent of immediate perception are rooted in perception, and, in truth, only lead us from one perception to another. This fact will become clearer when we come to discuss in the next chapter certain categories that begin in the sensory field, and lead out into the ideational field.

In our exposition we have stated that a child must *acquire* this or that form of behaviour. We have selected this vague term " acquire " expressly, because, as was pointed out in the second chapter, development may follow either of two paths—namely, maturation or learning. With reference to acquisition we must keep both of these in mind; for although learning is incomparably the more effective process of the two, and the one which therefore chiefly engages our interest, it would be a mistake to regard every acquired performance as necessarily one that has been learned.

§ 2—*Maturation and Learning. The Problem of Memory and the Problem of Achievement in Learning*

Maturation becomes noticeable in the first weeks of life, mainly through the growth of the " new " brain as it gradually becomes more effective in its functioning. Among other signs, this growth is indicated by a reflexive irritability which, though very slight at the beginning, increases until it attains its maximum within a few weeks, after which time it begins to decrease. The reason for this change is that as soon as the brain, and the connections between the brain and the spinal cord, have attained a certain stage of development, the cerebrum becomes a part of the system that produces reflex-action. Since it now issues from a new system, reflex-action takes on a new form. Goldstein has recently demonstrated that this change can no longer be explained by inhibition.[154] The transformation of the Babinski-reflex into the plantar reflex, for instance (cf. p. 86), depends upon the maturation of these parts. When disease destroys the connections in the pyramidal tract between the brain and the spinal cord, the Babinski-reflex reappears in place of the plantar reflex. Similarly, the reflexive control of excretion depends upon a certain maturation of the cerebrum. In the anencephalic child previously described this control was never effected. Behaviour of this type, however, can not be regarded altogether as a product of maturation, for learning is also involved.

Learning, however, brings before us an entirely new set of problems, to which we must now give our attention. All learning depends upon *memory*—upon the fact that the past is not dead to us, but is preserved, more or less, in some form or other within our psycho-physical organism. Whenever we have adjusted ourselves to a new situation, or have once solved a new problem, we find that our behaviour is easier the next time we meet the same or a similar situation. This aspect of learn-

ing has been especially favoured by investigation, and numerous experiments have been carried out by different methods with the object of determining the laws of memory. The problem of memory is, however, not the only problem of learning; for still another problem has at least an equal importance. We have just stated that memory makes it possible for the organism to preserve the effect even of a single performance. Consider, now, this single performance a little more closely. If it be of an inherited type, such as an instinctive action, it need be no easier, nor succeed any better, the second time than it did the first; because instinctive activities are already fairly complete at the start, and even if a certain improvement is noted, this need not necessarily be referred to memory, for it may be entirely attributable to growth. We shall see in the course of this chapter that, as a matter of fact, the maturation of a performance is promoted by its exercise.

The superiority of subsequent performances over the first is evident, however, when the activities in question involve more or less serious difficulties of acquisition. We may cite examples from each of the four types of development that we have distinguished. 1. Swimming is learned with considerable difficulty; once learned, however, we need never afterwards be quite helpless in the water. 2. Having once solved a puzzle-picture, the next time we see it the solution is very much easier; this facility attaches also to other pictures similar to the first. 3. Having once succeeded in crossing a stream on a log, one is not likely to hesitate as to what to do the next time one finds oneself in a similar predicament. The example of the burned child, which we have also referred to this type of behaviour, seems to be of a different sort, but we shall defer consideration of it until later. 4. After I am once able to understand a proof in some particular field of mathematics, I find myself much better prepared with respect to other problems in the same field.

These are all significant instances of learning, and in all of them the first performance contains the determining factor. For the psychologist, the problem of learning, therefore, is not merely one of finding out how later performances depend upon earlier ones—which is essentially the problem of memory—but also involves the question: How does the first performance come about? Hereafter we shall refer to this as the *problem of achievement.*[155]

The distinction here made is a fundamental one, although it has not usually been accorded the important position in psychology which it deserves. Often, indeed, the problem of learning has been identified with the problem of memory, while the problem of achievement, as a matter for separate consideration, has been more or less overlooked. Thus the criterion of an instinctive performance has frequently been found in the fact that it takes place without previous experience. Accordingly, whatever a living being does the first time it is placed in a certain situation is supposed to depend solely upon its inherited disposition.[156] This view we shall oppose by another which assumes all learning to be a nonheritable achievement. What this means, we must now endeavour to find out.

§ 3—*The Principle of Trial and Error. Thorndike's Investigations, and the Mechanistic Theory of Learning*

We now come to one of the most significant problems of comparative psychology, the solution of which is supposed by some to have been reduced to a very simple formula, namely, the Principle of Trial and Error. However, instead of untying the knots of the problem, this formula merely enables us to slip by them; for according to the hypothesis there is no such thing as a "non-heritable" type of behaviour, nor are there any first performances in the sense of being new

performances. It is important to bear this in mind when one is trying to understand the Principle of Trial and Error.

We shall begin by considering the concrete facts which have led to the formation of this principle. These facts may be found in typical experiments with animals, such as Thorndike was the first to undertake, and which have since been carried out very extensively in America.[157] A general idea of these experiments may be had from the following statement: Animals that have not for a long time been fed are confined in closed cages before which food is visible, or otherwise perceptible. Observations are then made of the behaviour of an animal in this situation, and especially of how it finally succeeds in getting out of the cage to the food.[158] The cage is provided with a door or some other contrivance which opens as soon as the animal has carried out a certain act, the animal being required to pull a string, or turn a lock, or press upon a board, or by means of some other mechanical device raise a latch, so that the door can be pushed open, or release secured in some other manner.

Fig. 6, taken from Thorndike's book, shows in a

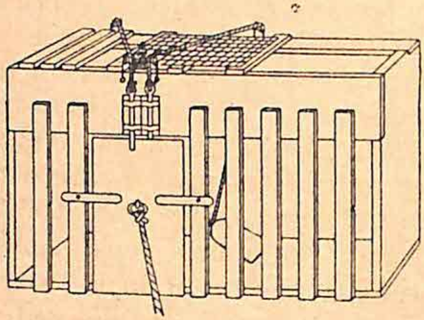


FIG. 6. [*After Thorndike.*

schematic way how such a cage is constructed. Among the many different locks pictured, a particular experiment may employ either one only, or one combination

of several, leading in a definite serial order to the release. For instance, it may be impossible to loosen lock C until B has first been unlocked, and lock B only after lock A.

Thorndike, whose experiments we shall now trace a little more closely, confined cats and dogs in such cages, always using each animal alone. He then observed what the animal did under these conditions, and measured the time from the beginning of the experiment up to the moment when the animal succeeded in getting out of the cage. Sooner or later, after the animal had eaten, it was again placed in the cage, and the experiment was begun anew. The repetitions extended sometimes over several days before the animal could at once release itself from the cage. The time of confinement in each separate test having been measured, a time-curve could then be constructed in which the repetitions are indicated on the abscissæ, and the time required in each repetition on the ordinates (cf. Figs. 7 and 8, pp. 180, 182).

Of course, it may happen that the animal will never succeed in escaping from the cage, but as soon as it is confined the animal begins to show signs of distress and to strive for relief. Thorndike's description of this behaviour has already been given on page 99. The procedure continues until the animal, in the course of its aimless pursuit, at length chances to make the movement which gives it freedom. Thus an animal striking about at random may sooner or later fasten its claws upon the string, or upon the bolt, that affords a means of exit. The animal thus gains its freedom the first time by a movement in no wise new, being one that already belongs to its inherited repertory of reactions (cf. p. 99).

If the experiment is repeated again and again, the behaviour of the animal changes, in that the unsuccessful movements are gradually reduced in number, while the successful movement becomes constantly more precise

and perfect ; both results have the same influence upon the time-curve, which shows that the animal gets out of the cage quicker and quicker.

These are the facts. How are they to be understood ? American animal psychology prides itself with having worked out a very simple hypothesis. This hypothesis has passed through different stages, some of which we shall here recapitulate, but its nucleus was given at the start as a result of the following considerations. Since insight and intention play no part in determining the movements by means of which the animal frees itself the first time from the cage, these can be no more effective after the animal has learned to master the situation, and hence the modification of behaviour by the elimination of the useless and the perfection of the useful movements may be said to go forward *without any participation on the part of the animal.* The animal has not the slightest notion why its behaviour is being modified ; the whole process, in which the successful acts are preserved and the unsuccessful acts gradually eliminated, is purely mechanical.

This is the Principle of Trial and Error, or Success and Failure. But the question remains : How does it happen that the successful movements rather than the unsuccessful ones are retained ? The first answer given to this question was that a definite connection, or association, is gradually built up between the situation and the useful movements, in consequence of which the perception of the situation is immediately translated into appropriate activities. An association is established between the situation and the appropriate, but not the inappropriate, movements, because the former are attended by pleasure, whereas the latter are attended by displeasure. This, approximately, was the theory of Lloyd Morgan. But the further questions how pleasure and displeasure can be effective in establishing or hindering associations could be answered by Morgan only thus : " I conceive," he says, " that there is but

one honest answer to these questions. We do not know." [159]

For a long time the theory remained in this form. Even recently, Bühler, in his attempt to explain the principle of training, or drill—which we shall consider later—appears to accept this view; he remarks that the pleasure of success and the displeasure of failure suffice to establish "an unequivocally clear and definite association between certain sensory impressions and the movement-complex of the successful mode of behaviour." [160] This connection is assumed to be purely associative; that is, the sensory impression determines the movement without the animal's being conscious of an "I should," or an "I will." [161] Morgan's theory has therefore been modified to this extent, that the association is now supposed to be established directly between the perception and the movement without the mediation of any other conscious data. Thorndike accepted this view at first, and proceeded to verify it by experiment. According to his first hypothesis, the association was supposed to take place only in the connection between the sensory impressions and the movement-impulse of the animal under investigation.[162] Let us see what is involved in the employment of this word, association.

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By association we understand a connection between processes not inherited but acquired in the course of life. The term has this meaning for Morgan, and also for Bühler, who writes: "There is an 'over-production of movements' and an 'aimless trying-out' involved in training. Consequently the range of possibilities is sufficient for the attainment of an end by chance. This range of chance is restricted, however, and finally set aside altogether by the building up of an unequivocal association." [163] If one understands by "over-production" the appearance of movements not connected by any inherited pathways with the situation at hand, it follows that new bonds of connection must actually be established.

Thorndike, however, sees the matter differently. "Over-production" to him is only the successive functioning of inherited modes of behaviour. As already remarked, the animal, according to Thorndike, does nothing at all to secure its freedom which is not already a part of its instinctive tendencies, and is wholly dependent upon the predetermined inherited connections of its neurones. The connections established in learning are, therefore, in no wise new; the total effect consists only in this, that among the numerous predetermined bonds existing between any situation and the many possible reactions to it, a few are retained and strengthened while the rest are eliminated. Although Thorndike does employ the term association, this function does not signify for him the establishment of any new connection in a physiological sense, but at most a facilitation in the functioning of nervous tracts already defined.[164]

The same view is advanced in its most extreme form by Watson, who is very emphatic in stating that there is no such thing as building up a new course of action, and that to speak of association is therefore quite superfluous. We need not concern ourselves at all, he thinks, with the establishment of new connections, but only with a selection from among those already present, and this selection results from the mere fact that useless movements are gradually eliminated, whereupon the useful ones fall into their proper serial order.[165]

Learning could not be more completely reduced to mechanical terms. Even the questions how the selection among different ways of response is to become effective, and what factor gradually determines the elimination of the useless movements, have been answered by Watson in the simplest, but also in the crudest, and, as regards a natural feeling for living creatures, in the most unsympathetic manner.[166] (The movements retained he regards as being merely those most frequently carried out; these being at the same time the successful ones for the simple reason that they are movements which

must occur in every trial that does not end in failure. No such compulsion attaches to the unsuccessful acts, because the experiment ends as soon as the right act has been performed. If one assumes that all possible acts are equally probable at the start, and that one order of acts is as probable as any other, it follows that in practice the right act has double the probability of any act that is wrong.

A simple diagram will clarify this relation. Suppose only two movements, A and B, are possible and equally probable, and that B leads to the result, while A does not. Then the series of trials may be something like this:

1. A *B*
2. *B*
3. A *B*
4. A *B*
5. *B*
6. *B*
7. A *B*
8. *B*

Whenever A comes first B must follow, but when B is the first member there can be no second, because B closes the experiment. We see that B occurs in the eight trials eight times, while A occurs but four times, although as the first member one is just as frequent as the other.

This Law of Frequency became for Watson and other American authors [167] the chief law of learning. Watson supplemented it with the less important Law of Recency, according to which the act last performed has a certain advantage over the others, which enhances the probability of its reappearance. Being always the last in every trial, the successful act is, at the beginning of each succeeding trial, the one most recently performed. But the original principle of explanation, whereby the effectiveness of success and failure was referred to

pleasure and displeasure, has altogether disappeared from the theory, and is no longer regarded as having anything to do with learning and habituation.[168]

This extreme point of view has not proved acceptable to the majority of investigators Although all recognize the Law of Frequency, or, as Thorndike calls it, the Law of Exercise,[169] it is not generally thought that this law alone is adequate to give a full explanation of the facts. Explanation in terms of the result itself, which Watson discards, is therefore retained by other investigators as a necessary addendum. Thus Bühler finds pleasure and displeasure the effective means by which infants and animals select their responses. "Success brings pleasure, and pleasure determines the frequent repetition of any movement that was once successful, while frequent repetition gives it a fixed and enduring character. Failure, on the other hand, brings displeasure, which does not prompt repetition. Thus unsuccessful movements are not retained, but eliminated." [170] The process of " stamping in " is therefore explained by frequency, but the frequency of the act is again referred back to pleasure. This seems very simple at first, but difficulties arise as soon as one considers a concrete case, as, for instance, that of the animal experiments just described. The connection between movement and pleasure, for example, is not nearly so close as the hypothesis would have it. A cat, while engaged in biting the bars of its cage, may gain its release by a chance-movement of its head which throws the lock. The subsequent pleasure in being free is supposed to be effective in determining a repetition of the same movement, but in order that this movement may again lead to success it must be repeated in exactly the same manner and in the same place ; otherwise the cat's head will not come in contact with the lock in such a way as to open the cage. But what causes the animal to assume this same position again ? In point of fact, as Hobhouse in particular has

observed, the animal does *not* repeat the same movement, but as a rule only the same general kind of behaviour. Thus a cat which has once freed itself by pulling a string with its foot, may upon another occasion pull the same string with its teeth.[171] The argument can be carried still further; for, if we accept Bühler's hypothesis and its consequences, the movement, strictly speaking, must be repeated exactly as it was made the first time success was achieved. But it is absurd, of course, to suppose that repetition will occur with any such exact or, as one might say, photographic fidelity to the original movement. An attempt to prove such a thesis must certainly fail. Indeed, so many elements of movement are present in the restless behaviour of the animal that the same succession of acts is quite impossible until the animal has learned its task, and the habit has been completely formed. In the case of the cat which secured its release by a movement of the head, the animal would certainly be found in a somewhat different position the second time the trial was made, and this would necessitate a somewhat different movement in order to slip the lock. The art of learning simply can not be explained by the mere repetition of a movement which leads to pleasure.

The theory of trial and error meets still another difficulty which its opponents have pointed out. In order to fixate the right movement, the satisfaction must be retroactive; and there is no way of telling how far back the satisfaction can extend. It should be noted that other movements, including at least going to the goal, are introduced between the critical act and the achievement of satisfaction. The hypothesis altogether fails to explain why the critical act should be "stamped in," when the satisfaction attaches, not to it, but to the subsequent seizing upon and eating the food. Indeed, the pleasure often follows much later than the act, because a whole series of acts, some right and some wrong, may have to be made before the end

is attained. For instance, when the cage from which the animal is to release itself has more than one lock, the opening of the first lock can bring no pleasure, and before the other hindrances are set aside the animal may make many false responses. Yet even under these conditions the animal will learn to carry out the first act of such a series.

If the satisfaction of eating is responsible for "stamping in" the act which the animal is learning, then the closer satisfaction follows upon this act the easier it should be to learn it. Warden and Haas have tested this assumption in experiments with white rats in a simple maze. The rats were divided into two groups. One group was allowed to feed as soon as the maze had been run through and the food-box attained. For the other group feeding was delayed. A delay of five minutes before the food was given had no appreciable influence upon the rate of learning.[172]

We have not as yet criticized the Law of Frequency, but it is not difficult to demonstrate that it is inadequate, and likewise that its derivation from the law of probability is unfounded. Thorndike refutes the law very simply[173] by pointing out that the entire deduction is based upon a false presupposition; namely, that the animal will perform each separate act once only, and must then proceed to a new and different act—a procedure that does not at all agree with the facts. Very often an animal will repeat an unsuccessful act many times before a change takes place in its behaviour. In these cases repetition would have a quite different result. Consider the previous illustration, where there were but two possibilities of reaction, A, unsuccessful, and B, successful. B can be repeated but once in a trial because the first B solves the problem, whereas A can be repeated many times. With a similar scheme to that used on p. 175, but allowing A to be repeated three times before the act is abandoned,

we gain the following picture of the animal's behaviour in successive trials :

1. A A A *B*	5. *B*
2. *B*	6. *B*
3. A A A *B*	7. A A A *B*
4. A A A *B*	8. *B*

From this record it appears that A has occurred twelve times, while B has only occurred eight times. By the law of frequency A rather than B should be selected, which shows clearly the inadequacy of this law as an explanation of learning.

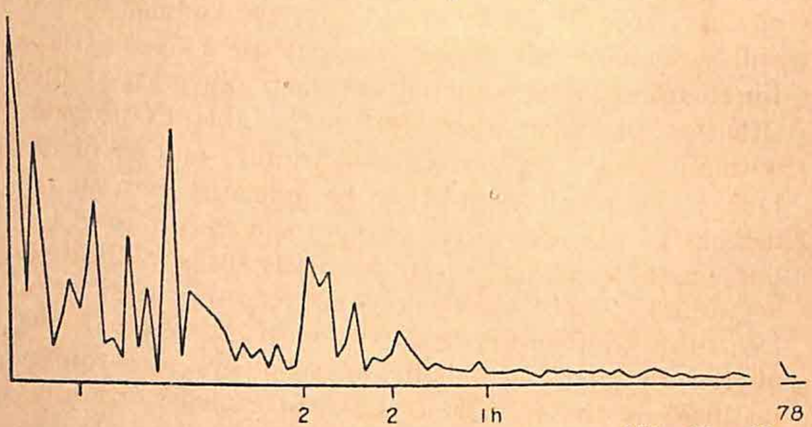
Thorndike attempts to overcome this difficulty by adding to the Law of Exercise, a Law of Effect.[174] If a reaction leads to a " satisfactory state of affairs," the connection involved in the reaction is strengthened, whereas if it leads to an " unsatisfactory state of affairs," the connection is weakened. This addition is nothing more than the old principle of the effects of pleasure and displeasure now reduced to an original innate tendency ; but why the principle should be effective, we can understand no better than we did before. Thorndike, however, tries to set this question aside altogether, by basing the Law of Effect upon the individual's inherited tendencies.

The same objection already raised against Bühler's formula can, however, be applied to Thorndike's principle as soon as we take it up in detail, and trace its consequences as they are applied by Thorndike in explanation of the learning of animals. But before we follow this criticism further, I wish to point out that, to me at least, it seems as if Thorndike himself were not altogether satisfied with the dominating mechanistic tendency of his principles, and that he would like to overcome this implication by means of the Law of Effect. At any rate, he also considers the ethical aspects of development, and he clearly refers the possibility of ethical progress to this Law of Effect

when he writes: "Man is thus eternally altering himself to suit himself. His nature is not right in his own eyes. Only one thing in it, indeed, is unreservedly good, the power to make it better. This power, the power of learning, or modification in favour of the satisfying, the capacity represented by the law of effect, is the essential principle of reason and right in the world." [175] Since we shall be obliged in what follows to criticize Thorndike's principles adversely, it seems only fair to note the tendency he has seen fit to incorporate into an otherwise mechanistic hypothesis.

§ 4—*Thorndike's Hypothesis Criticized by Showing that the Behaviour of his Animals was not altogether Stupid*

Let us now return to Thorndike's theory of learning, according to which acts "teach themselves," so to say, inasmuch as the animals never participate in what they



[*After Thorndike.*

FIG. 7.

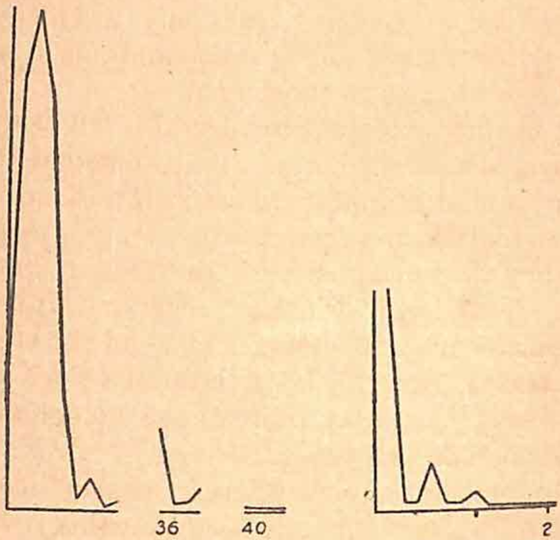
are doing, and never know that a critical action will bring them freedom and food. Since this assumption of animal stupidity is at the root of Thorndike's whole theory, we must first of all test it out. In the main, Thorndike derives the proof of his radical thesis from

two groups of facts: the time-curves of the performances, and the errors the animals commit.

The time-curves, which have already been described (p. 171), are so constructed that one millimetre on the ordinate is equivalent to ten seconds, the small marks upon the abscissa indicating interruptions in the experiment. Unless otherwise noted, these interruptions represent a whole day. When several days elapsed before a new trial was made, the number of days was indicated near the mark; or if less than a day, the number of hours was indicated by the addition of a letter *h*. The curve here reproduced (Fig. 7) is typical of the performance of a cat, which, in order to secure its freedom, had to turn a movable wooden bar-lock from a horizontal into a vertical position. (Locks of this kind are shown upon the door in the picture on p. 170.)

Thorndike argues that if the cat possessed a trace of intelligence, it could not happen that, after having already freed itself several times, the animal would still be unable to repeat the act in a later trial. Furthermore, if the animal ever actually grasped the situation, it ought thereafter to be able to proceed without delay to a correct and definite solution of its task. This result would then be indicated by a sharp descent of the time-curve without any recurrent rise; but, on the contrary, the time-curves always indicated a gradual descent with numerous recurrent rises. So far as this argument is directed against the explanations offered by an "arm-chair" psychology, it is quite justified, for the animals in these experiments certainly showed no "consecutive thinking." Yet, in declining to accept an anthropomorphic explanation, we are by no means required to assume that all animals exhibit a complete lack of insight. For one thing, many of the curves do actually show the sharp descent demanded by Thorndike as a criterion of insight. Two such curves relating to the same problem as the first curve, are here reproduced (Fig. 8).

These curves show not only a sharp descent, but no recurrent rise even after a long interval of time; a result which also contradicts the law of exercise, since a long pause ought to weaken the bonds previously established (cf. pp. 175 f., and note *169*). Why should we not proceed from cases like these, and lay our emphasis upon the suddenness rather than upon the gradualness of learning? Thorndike himself found sudden progress to be typical in the learning of monkeys;



[*After Thorndike.*

FIG. 8.

and yet the gradual type of learning, which characterizes these earlier experiments, seems to have so strongly impressed him that he dismisses sudden learning with the remark that, "of course, where the act resulting from the impulse is very simple, very obvious, and very clearly defined, a single experience may make the association perfect, and we may have an abrupt descent in the time-curve without needing to suppose inference." 176 But the position he takes is open to objection, because the description of a solution as "simple," "obvious," and "clearly defined" can apply only to

the experimenter and not to the animal. According to Thorndike's own presuppositions, the animal does not participate at all, nor does it even understand the solution after it has been mastered; and hence there can be no point in saying that the solution is "obvious" to the animal. The time-curves we have reproduced will indicate how differently different animals behave in the same situation; yet Thorndike is unable to refer to individual differences, because the individual has been excluded from his theory. Therefore, whatever is "simple" or "obvious" can only include what is "objectively" simple or obvious, and not at all what is simple or obvious to the animal.

That in these experiments a sudden fall in the time-curve ever should occur, and that it sometimes happens that an animal is able to master its task in a single trial, are matters that can not be simply brushed aside when they do not agree with the Law of Frequency, in accordance with which a long and troublesome development must be assumed, even for the objectively easiest tasks. Since in the initial trial a single response must always be selected from among a large number of equally possible responses, the law of effect is, therefore, the only one upon which an explanation can be based, and we have already seen that this law is itself in grave need of elucidation.

As a matter of fact the ability of an animal to learn an act by performing it a single time is not at all unusual. Lloyd Morgan in his observation of fowls has reported instances like the following: He brought a chick seven days old into his study, and placed it in a pen made of a newspaper. The chick began to peck and scratch at one corner of the pen until it made an opening, and was able to come out into the room. When caught and replaced in the original position, the chick ran to the same corner, and again pulled down the newspaper, and came out into the room a second time. The chick was then placed on the opposite side

of its pen, but it soon returned to the first corner, and released itself a third time in the same way.[177]

In behaviour like this the inadequacy of Thorndike's principles is keenly felt. It seems nonsensical to suppose that the breaking down of a certain corner of the pen should have nothing to do with the chick's release from the enclosure. Furthermore, the fact that on the third trial the chick ran back to its original corner can only be explained as a matter of chance by Thorndike. Since all that could have been learned in a blindly mechanical fashion was the movement of pulling down the paper, the procedure of the chick to a particular place in the pen could not be included in the original response.

In short, the conclusion that animals are altogether blind in their learning is not sufficiently evident from the time-curves. Nor is the argument Thorndike bases upon the errors committed by animals any more convincing. Animals that have completed a certain performance one or more times frequently fail in later trials, or act otherwise than they would if they really understood what they were about. "Stupid errors," as Köhler calls them, have often been reported in animal experiments. Cats have been observed to strike at strings or at levers when the door of the cage was already open. Sometimes they will strike at a certain place after the device which once called for this action has been removed.[178] But must we accept a purely mechanical hypothesis because it can be shown that some acts are not fully comprehended? This question assumes greater importance when it is associated with another, namely: Has the experimenter selected the conditions of his experiment in such a way that the animal could possibly have understood what he was about?[179] A mere glance at the picture of the puzzle-box on p. 170 will suffice to answer this second question in the negative. Without possessing some technical experience, even a man placed inside of such a box

would be unable to comprehend these mechanisms of release; for several essential parts are placed on the outside, and are therefore invisible from within. Accordingly, the connection between the movement made and its effect upon the animal must necessarily be of a purely arbitrary sort. Even in the employment of the simple turning-bar lock which produced such good time-curves, the experimenter did not raise the question whether this lock could be understood by the animal tested. Yet, unless one knows this, one is quite unable to decide where the difficulties lie, and what actually constitutes the animal's achievement in overcoming them.

In this connection it should be mentioned that stupid errors do not always occur, even when the conditions seem peculiarly favourable to them. In an ingenious experiment performed by Higginson, rats were trained by 100 repetitions each to run a certain maze which included several blind-alleys. Since a rat could ordinarily learn this maze in twenty-five trials, the "habit" which was here established by overtraining should have been exceptionally strong. After the period of training a door was opened in the maze which allowed the rat to cut out one of the alleys and thus reach the food-box more directly. Among the nine rats of the experiment, five changed their responses at once and took the short cut to the goal, while the other four did the same after a slight hesitation.[180]

The results of Thorndike's experiments with monkeys led him to essentially the same conclusions that his previous work with cats and dogs in puzzle-boxes had seemed to warrant. It is worthy of note, however, that the evidence is now of a different sort. (The time-curves of the monkeys reveal sudden learning, and no stupid errors are reported.) Thorndike's chief reasons for supposing that the learning of monkeys is also blind are based upon lack of progress under the influence of tuition and imitation. Since a discussion of these

influences is reserved for the next chapter, we shall omit further comment on these additional features of animal learning, and continue with the simpler form of individual learning.

Before we proceed with our criticism, however, certain facts should be mentioned which are recorded in Thorndike's earlier experiments, and are substantiated by other investigators. It can be shown, for instance, that animals that have already undergone a certain experimental training are better fitted to meet the somewhat varying conditions of similar tests than other animals that are being experimented upon for the first time. This is undoubtedly to be explained in part by the fact that the new situation of being locked up in a box gradually loses its terrifying effect upon the animal; accordingly, as the animal becomes less excited, it makes fewer aimless movements. If we compare the time-curves of Fig. 7 with those of Fig. 8, which relate to the same problem, their difference may in part be attributed to this influence; because the first curve is that of an animal learning the act of turning a wooden bar in its first puzzle-box experiment, whereas the two other animals, the time-curves of which appear in Fig. 8, had already been tested in other boxes where the task involved striking, biting, or rubbing against a wire noose hanging some fifteen cm. above the floor.[181]

In addition to the general effects of previous experience, certain more specific influences can also be demonstrated. Modes of procedure that prove to be unsuccessful, such as biting at the bars of the cage, or attempting to force the body through too small an opening, are less frequently employed as the animal becomes more experienced in the tests. All these facts can be readily understood in accordance with Thorndike's principles, and would naturally operate to shorten the learning-curve.

It is a different matter, however, when we come to

another modification reported by Thorndike, namely, "that the animal's *tendency to pay attention to what it is doing* gets strengthened; and this is something that may properly be called a change in degree of intelligence." [182] But how can this statement be reconciled with the assumption that animals have not the slightest knowledge that their actions have anything to do with their achievements? Why, we may ask, do they give attention. And, above all, why does Thorndike use the word *intelligence?*

The facts upon which his statement rests are highly significant. After having once learned to free themselves from the first box by striking at a noose hanging from the front wall of the cage, both cats and dogs were found to require much less time in freeing themselves from a second box in which the noose hung from the rear wall. In the case of a particular dog replaced in the same box after an interval of a day or so—the noose being now hung considerably higher than it was before—the problem was virtually solved at once; the first three trials lasted but twenty, ten, and ten seconds, respectively. "After nine days he was put in a box arranged with a little wooden platform two and one-half inches square, hung where the loop was in the previous experiment. Although the platform resembled the loop not the least, save in position, his times were only ten, seven, and five seconds." We have, therefore, in these cases a true *transfer of training;* for the animal employed a procedure which was successful under certain conditions, after these conditions had been altered, and he did so in a manner appropriate to the alteration. One might suppose that this would make difficulties for a strictly mechanistic theory of interpretation, but Thorndike believes these difficulties can all be set aside without altering his hypothesis in the slightest. Thorndike objects, quite rightly, to an obsolete psychology which would infer from such observations that the animal must possess "general

ideas" ; that he must have understood, for instance, that to strike at a loop would bring release, or that " this thing in my cage is a loop " (though, to be sure, the external form of the loop was altered in many of the tests without disturbing the effects of practice). It can not be supposed that the animal is able to infer that a certain object must be struck at, no matter whether it hangs in front or behind, high or low. To an hypothesis like this one may rightly object, but at the same time Thorndike blinds himself to what such an achievement actually signifies as an evidence of transfer. Thorndike thinks, for instance, that the animal can not see the separate things of our world at all ; that he possesses only a vague total impression of the situation. Thus, a bird diving into the yellow water of a stream, or into a pool, or into an ocean, would not be able to see the difference that we would see in these situations. Only the total situation "water" comes into consideration for the bird ; consequently, in the experiments reported, " the loop is to the cat what the ocean is to a man when thrown into it when half asleep." [183] On the other hand, when a human being is confronted with a task, the total situation is at once broken up into its elements, among which the important ones appear in the foreground. This reduction simply does not take place in animals. Instead, it is the total situation, including its undifferentiated parts, which connects itself with the impulse of response, and this connection is influenced neither when one adds elements to the situation nor when one subtracts them ; provided only *that something is left which is capable of arousing the impulse.* Hence, to Thorndike, the fact of transfer indicates, not mental progress, but, on the contrary, a very primitive and undifferentiated stage of development.

This is Thorndike's argument, but it is self-contradictory ; for in the first place the total situation with all its elements is supposed to be connected with the

impulse ; while in the second place we are told that the situation can be enlarged or reduced at will, though, as indicated above by the phrase in italics, one element at least must remain unchanged, or the connection itself will be lost.

It is not our purpose to revamp the anthropomorphic hypothesis which Thorndike has so vigorously attacked. On the other hand, our conception of the primitive aspects of the phenomenal world can not be stated in terms of a number of separate phenomena, each clearly set off from every other ; but the facts of the case do not require that we should accept either this older theory or the one Thorndike has advanced. The vague total situation described by Thorndike is not at all what we have previously referred to as a configuration, however primitive ; for the primitive configuration as we conceive it is not a single vague total quality, but a "quality upon a uniform ground." Neither do we find Thorndike's "vague total quality" applicable in the explanation of any true transfer of training. Indeed, if "stupid" errors such as we have described occurred more often than they do, the theory of the total situation would be in a better way. If the animal in a puzzle-box, with the loop now hanging behind instead of in front, were directed only in accordance with the vague total situation, it would be forced to strike forward in the direction where the loop previously hung ; and all the more so because its natural behaviour would prompt it to attack its goal directly, rather than turn aside as it must do in order to reach the loop which now hangs at the rear of the cage.[184] Yet instead of following this natural tendency which would attract it to the front of the cage, the animal usually alters its behaviour to correspond exactly with the alteration of the most *important* feature of the situation. Is not the inference justified that, in so far as the animal has learned to free itself from the first box, it has also learned to re-organize the situation in a definite and more or less

detailed manner? If so, the same configuration will again be effective, even when the loop is hung in a different position in the second box. And hence "stupid" errors, or responses carried out without reference to existing conditions (as when the animal strikes at a loop in a position where it no longer hangs), would appear to be essentially different from behaviour in which a real transfer of training is manifest. Thus it is only the "stupid" errors, but not the efforts of reconstruction in similar situations, that indicate a low degree of capacity for achievement.

To explain a positive performance which happens to involve something more than one was led to expect of it in terms of an inherent deficiency is always a questionable procedure. We ought, therefore, to proceed methodically, and allow the experiment itself to determine whether the animal's performance is to be regarded as an evidence of inherent incapacity or progressive achievement. Even Thorndike's experiments seem to show, not only that the animal experiences certain vague total situations, but that, in the course of learning, this total situation becomes organized. When the loop differentiates itself, it is not as if it were now seen as a circular or elliptical figure of definite magnitude and colour; it is merely "something to be struck at," or "something to be moved." As such, it becomes the central feature in the total phenomenal situation. This situation, however, is essentially characterized for the animal as a "situation from which I wish to release myself in order that I may get at the food which lies outside." If, now, the loop becomes the central feature of the situation, this shows that neither it nor the movements made with it are without significance to the animal; for the animal has in some way connected its action upon the loop with the food outside the cage. The theory of an entirely meaningless learning is simply untenable.

The phenomenal description of the loop as "some-

thing to be moved" calls to mind a description employed in the preceding chapter when the distinction was drawn between a "transitional" and an "end-situation." The loop comes to possess this "transitional" character, and to it there accrues a certain definition of the kind and the manner of this transition. In other words, the perception of the object has undergone a significant change; the loop which first of all was more or less vaguely involved in the total situation now, as a result of learning, gives rise to a new phenomenon. This transformation could not result either from mere association or from a mere increase in the permeability of an already existing connection. We shall soon have something to say against the principles underlying the entire associational hypothesis. A further discussion of this matter can therefore be deferred until we are ready to take up the question in greater detail. But we have already achieved an important result; for inasmuch as the loop has acquired a definite transitional character, something actually *new* must have occurred in the animal's experience; or, more generally stated, the learning accomplished in Thorndike's experiments has led to the creation of a new perceptual phenomenon.

Even from Thorndike's own results we can see that the facts of the case have not been forced in order to make them fit our theory. In an experiment with seven cats, tests were made of a different sort from those previously described, and with quite different results. In these tests the animal was not allowed to free itself, but the box was opened by the experimenter as soon as the cat had either licked itself, in the case of four of the animals, or scratched itself, in the case of the other three. This experiment was also successful, and it is of the greatest interest to know "whether animals react differently to experimental situations which involve a partial possibility of intelligent behaviour than they do to such as involve none—for the difference,

if any, is obviously crucial." [185] The behaviour *was* different. As Thorndike describes it: "There is in all these cases a noticeable tendency, of the cause of which I am ignorant, to diminish the act until it becomes a mere vestige of a lick or a scratch. . . . Moreover, if sometimes you do not let the cat out after this feeble reaction, it does not at once repeat the movement as it would do if it depressed a thumb-piece, for instance, without success in getting the door open. Of the reason for this difference I am again ignorant." [186]

Köhler points to this as one of the most interesting of Thorndike's results. We might describe it as follows: The behaviour of the animal is typically different when the movement by which its freedom is gained is objectively meaningless. When the act has no sort of internal connection with release, the behaviour is not the same as when the movement leads directly, even though in an obscure fashion, to its end. The difference in the animal's behaviour corresponds with the difference in the conditions imposed, indicating that in the two cases the critical act is introduced into the animal's experience of the situation in different ways; which means that the act must somehow have something to do with the situation as the animal experiences it, and leads us to assert that, with vertebrates at least, there is no such thing as an entirely meaningless learning.

This conclusion is confirmed by one of McDougall's experiments.[187] Before the eyes of his dog, McDougall placed a biscuit in a box which he then closed. The lid of the box could be opened with comparative ease by pressing upon the handle of a lever. Later the experiment was made more complicated, though all the complications were simpler in character than were the contrivances of Thorndike's puzzle-box. From these experiments McDougall infers that "while the dog's behaviour was from the first purposive, . . . the goal and especially the steps toward the goal, became more

defined in the dog's mind as he became more expert in his task." Among the data upon which this conclusion was based, the fact is recorded that, after having once learned the task, the dog never repeated a fixed habitual series of movements, but with widely varying movements always achieved the same end. Tolman, who has investigated many phases of animal learning, summarizes the matter by saying that "all learning is *problem-solving*." [188]

§ 5—*Ruger's Comparative Tests on Human Beings*

We can now continue our discussion of "learning by trial and error," by asking how a human being would behave if he were confronted with a similar task. This question has also been investigated in America, and it is easy to understand why H. A. Ruger,[189] who undertook the problem, should have been led to do so after an investigation of animal behaviour which he had previously carried out under Thorndike's direction. Ruger did not need to confine his human subjects in cages in order to force them to exercise their powers by an impulse to seek freedom and food. The good will which they brought to the solution of their problems, strengthened by a desire to solve them as well as they could, furnished an adequate substitute for the more elementary impulses of lower animals. The problem in his case was to solve a mechanical puzzle. The observer received a wire-puzzle, and was instructed to remove some part of it. The time was measured from the beginning of the test until the puzzle was solved. The experiment was then repeated, always measuring the time, until the solution took place at once. The puzzle consisted of rings or other devices of wire strung together, the experimental subjects being called upon to find out which element of the group could be released, and how this might be accomplished. In comparison with Thorndike's dogs and cats, these

human subjects had a very great advantage, inasmuch as their problem was far more definite than that of the animal whose single object is to get out of the puzzle-box. Nevertheless, a measure of similarity exists between the two types of experiment ; for in neither case was a comprehensive understanding of the mode of solution possible at the outset. This lack of insight was heightened by the fact that the puzzles were three-dimensional devices which most of the observers found themselves unable fully to comprehend. At the same time, we know that man desires understanding, and that to him understanding is as much an end as the solution itself ; whereas in the case of a caged animal the only desire is to be released. (Despite all this, the procedure adopted by human beings in solving these puzzles often very closely paralleled the methods employed by the animals of Thorndike's experiments.) " The times for repeated success in a number of cases remained high and fluctuating, the time for later trials in a given series being often greater than that for the first success. . . . In practically all of the cases, random manipulation played some part, and in many cases, a very considerable part, in the gaining of success." [190] Naturally, connected operations of thought also occurred, and these were accompanied by an abrupt descent in the time-curve, without a subsequent ascent. Thoughtful operations, however, were not the rule ; indeed, the behaviour could at times be so stupid that manipulations leading to no change at all in the situation were nevertheless again and again repeated. We can see from this how hasty were Thorndike's inferences ; for inasmuch as his chief argument is based upon the time-curves, and upon the " stupid " errors, it ought to be possible to transfer his conclusions directly to human behaviour. On the contrary, untalented as a person may be for this sort of task, he must at least be credited with knowing that his movements have something to do with the solution of the problem at

Nature of task

hand. Consequently, if human behaviour is in many respects similar to that of other animals, one has no right to draw the extreme inference that animals lower than man possess no insight whatever. The experiments with human beings have one great advantage over those with animals in that the subjects experimented upon can give information as to how the thing was done. We can thus obtain more or less complete information regarding the internal behaviour of the subject, and are not solely dependent upon inferences. If we ask, then, "What constitutes learning in these experiments?" the answer is that, in addition to the mere perfection of manual dexterity, learning consists essentially in an *organization* of the whole procedure. Let us eliminate the few cases in which the solution was reasoned out, and follow this process of organization in the other cases. If a successful movement comes about by chance, as a rule the first consequence is this, that the region in which the work is being done, or the particular kind of movement that is being made, is now emphasized, and becomes the focus of the whole procedure. In a large number of cases the solution, therefore, is almost entirely a matter of "locus-" or "place-analysis"; that is, the subject now knows where he has to work. Thereafter a marked descent is recorded in the time-curve, without subsequent rise. Instead of the gradual elimination of irrelevant movements which had previously been carried out, we find the sudden exclusion of a considerable number of these. Ruger quite justly remarks that many of the sharp nicks in the time-curves of animals may likewise be attributed to this same factor.

What was found true in this very simple case also appeared to be true in more complicated instances. New variations of movements which proved to be successful occurred much oftener unintentionally—by chance—than intentionally. Their influence upon the time-curve, however, depended directly upon the kind

of consciousness given to these "fortunate variations"; that is to say, a new movement which brings success remains in the actual possession of the individual, so that it can be applied a second time, *only* when it has occurred in such a way that its significance has been recognized. The deeper the insight, the stronger is this influence; a result which, as we shall see, has no significance at all in purely motor learning, but is of great importance in passing judgment upon the behaviour of animals.

The nature of the subject's understanding is described by Ruger in detail. It is not at all a process limited to human *ideas*, but is one that may be entirely *perceptual*; in which case the field of perception undergoes a transformation, often sudden and profound, without in any way involving the introduction of "ideas." The motor side of the performance is naturally influenced thereby, so that the activity becomes adapted to the newly formulated field of perception. Thus this organization includes both the perceptual and the motor sides of the behaviour; but the completeness of the organization may greatly vary. At the lowest level, the whole process remains but a series of arbitrary steps, taken one after another. The unity becomes closer when these steps follow one another rhythmically; and at the highest level the activity is unified from beginning to end in the realization that a task is being fulfilled.

We may infer from this description that some degree of organization is present also in the experiments with animals, and that animal behaviour is not merely an objective succession of events.

In Ruger's cases "transfer of training," or the successful application of a method learned under certain conditions to other and different conditions, always presupposed understanding. One of Ruger's experiments substantiates this statement on the negative side. An observer was tested with a certain puzzle once, and then all the separate acts necessary to its

solution were extensively practised in systematic order. The same puzzle was then given to the observer in the same way in which it had been given the first time; but failing to recognize that the practised movements had anything to do with it, the movements he had learned were not applied, and his results showed that he was no better equipped than if he had not had the practice at all. This experiment also indicates that the organization of the motor and the perceptual parts must be undertaken together.

On the other hand, it was frequently observed that a certain practised procedure readily broke into another procedure, even when the subject knew perfectly well that it was entirely irrelevant to his task. This tendency of certain methods to "persevere" deserves special consideration, not only in view of what has already been said, but also in connection with certain experiments upon animals which we are about to describe.

From Ruger's experiments we have gained some insight into the behaviour of human beings in situations which at first were more or less obscure. It has been shown that improvement in efficiency goes hand in hand with an increased insight into the nature of the task. We use this word, *insight*, without theoretical presuppositions, in the common sense in which every one takes it. If one knows that he is to remove a ring in a certain puzzle, and that in order to do so he must first move this piece and then that, and then turn the puzzle over and do something else, his procedure may be said to possess a greater degree of insight than the procedure of another person who simply goes ahead without any plan at all. But if one also knows that the ring is connected in such and such a manner with such and such parts of the device, and that these are again to be turned thus and so, his procedure will indicate a still greater insight. The conditions of Ruger's experiments were intentionally chosen so as to make them as like the animal experiments as possible.

For this reason, insight entered into them only as a result of a behaviour which originally „lacked this quality, a behaviour which in itself could lead to a successful termination of the test only by chance.[191]

§ 6—*Intelligent Learning. Köhler's Experiments with Chimpanzees*

Can experiments be so planned that the animal's behaviour will show insight without the aid of a chance-discovery? When we consider what this means, it at once becomes clear that both animals and children are well-adapted to such experiments. Adults, on the contrary, are not suitable subjects, because they bring to their tasks a set of ready-made methods which need only be transferred to the new situation; but how these ready-made methods originated it is not at all easy to determine. If, on the other hand, the problem selected makes such a transfer impossible, it is hard to find a suitable test; for usually a task of this order is far too difficult for an experiment. Hence the study of insight in its incipient forms can best be undertaken with children and animals.

In experiments with animals one should begin with those species in which the relatively best performances may be expected in the problems to be solved; thus our choice unhesitatingly falls upon the anthropoid apes. Accordingly it was an event of importance to science when the Prussian Academy of Sciences founded its station for the observation of apes upon the Island of Tenerife. While serving as Director of the station, Wolfgang Köhler devoted the major portion of his time to an investigation of this problem. Not only are his results of great scientific value, but for a mere description of the life of the chimpanzee, as he observed it, they are also of such unusual interest that his book is worthy of detailed study by all who have anything to do with the investigation and guidance of human

intelligence.[192] If chimpanzees are able to solve original problems, not merely by chance, but with insight, then the behaviour of these animals ought to throw new light upon the nature of insight ; for modes of behaviour that have become a matter of course with us adults may be expected to appear in a more plastic form in the life of an ape. If the simplest acts of intelligence can in this way be brought under experimental observation, the results should yield important data for theoretical purposes. With adult man, on the contrary, an investigation of the earliest and simplest acts of intelligence is no longer possible.

Since Köhler's experiments provide us with the kind of information we need, we shall find it worth while to examine them in detail. Indeed, they furnish us with a significant contribution to the solution of our chief problems, namely the nature of *learning* in general, and the origin of the first *problems of achievement* (cf. p. 169) in particular.

With Köhler, therefore, we raise this question : Do chimpanzees show insight in their behaviour ? His general plan of investigation was as follows : " The experimenter sets up a situation in which the direct path to the objective is blocked, but a roundabout way left open. The animal is introduced into this situation, which can, potentially, be *wholly surveyed*. So we can see of what levels of behaviour it is capable, and, particularly, whether it can solve the problem in the possible ' roundabout ' way." [193] The criterion of insight is found in the animal's capacity to select the indirect way unaided. With reference to the words in italics, the experiments were so planned that, in contrast to the puzzle-box tests of Thorndike, the animal required no knowledge of human contrivances in order to select the indirect means to the goal.

But may the selection of the indirect means still rest upon chance ? And is Köhler's criterion therefore a mistaken one ? These questions are unequivocally

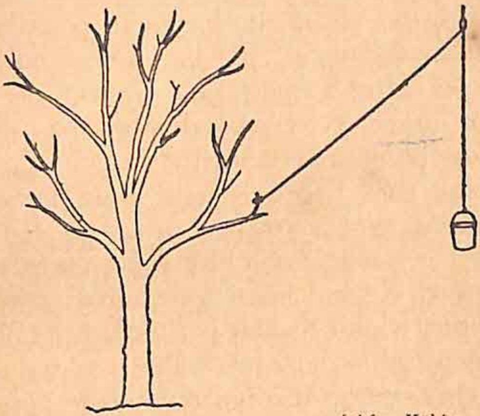
answered in the negative by the simple observation of what actually takes place; for the true and the chance-solutions are so entirely different in appearance that one has no trouble at all in reaching a decision as to which is which. In chance-solutions the animal runs now here, now there, each movement being independent of the preceding, so that only by a kind of geometric addition can we trace the curve of the path followed, beginning with the point of departure, and ending with the successful attainment of the goal. A true solution is quite different; for the animal proceeds by a single continuous curve from its original position to the attainment of the goal. To be sure, a true solution often follows after a perplexed period of trial and error; but in this case the difference is even more striking, for the animal suddenly gives a start, stops a moment, and then proceeds with a single impulse, in a new direction, to the attainment of the goal.

Examples of this sort will be given presently; but let us first note that what holds for animals also holds for children, upon whom Köhler performed parallel experiments, which Bühler has since supplemented.[194] In the case of a child one can often notice the very moment when the right solution first dawns upon him, by the way in which his face lights up. Such changes of expression were also noted by Köhler in his chimpanzees.

It was Köhler's rule to begin with the simplest problems, and to proceed systematically from these to the more difficult tasks. Only in this way can one be sure in a particular case which portion of the task was most difficult for the animal, and why this or that error was committed.

As his first test Köhler made the following experiment (see Fig. 9): An open basket containing fruit was suspended by a cord from wires crossing the top of the animal's cage. The cord passed through a ring, and the basket hung about two metres above the floor. The free end of the cord was then provided with a wide

loop, which was hung over a short branch of a neighbouring tree, the tree also being within the cage. This loop was about three metres distant from the basket, and at about the same height. As soon as the loop was removed from the branch the basket, of course, would fall to the ground. This may not seem to be an easy task, yet the situation as such is far more readily comprehensible than were those of the puzzle-box tests. As a matter of fact, the test proved much too complicated to begin with ; for the solution of Sultan, the cleverest



[*After Köhler.*

FIG. 9.

animal at the station, was made in the following manner : "After a time, Sultan suddenly makes for the tree, climbs quickly up to the loop, stops a moment, then, watching the basket, pulls the string till the basket bumps against the ring (at the roof), lets it go again, pulls a second time more vigorously so that the basket turns over, and a banana falls out. He comes down, takes the fruit, gets up again, and now pulls so violently that the string breaks, and the whole basket falls. He clambers down, takes the basket, and goes off to eat the fruit." [195] When the experiment was repeated three days later under slightly varying conditions, Sultan at once employed the last described method of solution.

We do not get very far with this result. To be sure, the animal has made use of the connection of rope and basket, but why no trace of the intended solution was indicated is not at all clear. Was it because the connection of rope and branch was not noticed, or was this connection incomprehensible to the animal? Perhaps the difficulty lay in the fact that the intended solution would have brought the fruit to the ground rather than into the hands of the animal, thus requiring the ape to employ an indirect means which at first would carry the fruit away from, rather than towards, him. That we can not answer these and other questions with any degree of certainty proves the inappropriateness of this experiment, and also the importance of the rule that one should proceed gradually from simple to more complicated tasks.

We shall now trace the course of Köhler's investigations, in order to review some of his more impressive examples that indicate what these animals can, and what they can not, accomplish. Köhler began with a method which was literally one of indirection. Slight indirections, such as overcoming obstacles, are constantly met with in the daily life of these animals. For the purpose of investigating somewhat more difficult modes of indirection, the following test was selected. In experiment No. 1 the basket was hung from the roof, but could not be reached from the floor. The experimenter then set the basket swinging near enough to a scaffold so that an animal who had climbed upon this could grasp the basket from his point of vantage.

In other experiments the connection between the animal and the fruit was made by an intervening link in the chain of behaviour. In the simplest case of this kind the connection was already provided by the situation—the question being: Can the animal make use of it? Thus, in experiment No. 2, the fruit was placed beyond reach outside the cage, but a string was

attached to it which came within the reach of the animal. In the next test (No. 3), the connection between the animal and the goal was not yet established, as it was with the string, but the situation included a stick within the cage as the only means whereby the animal could reach the goal. Similarly in No. 4, the goal was fastened to the roof, and a box was placed in the cage, which could be employed in order to reach it. A third variation of this method (No. 5), provided that the goal, which was hung aloft, could be attained if the animal would swing itself upwards by means of a rope hanging at a distance of two metres from the basket. All these experiments involve the *employment of tools*, by which we mean nothing more than the introduction of a mediating factor.

When reversed, this principle leads to the *setting aside of obstacles*. In experiment No. 6, the fruit lay outside the cage, with a stick for reaching it conveniently at hand. Within the cage, and directly opposite the goal, a fairly heavy box was placed, which prevented the animal from employing the stick successfully.

A new difficulty was then introduced: In order to attain the goal a tool is required which is not immediately available, but must be supplied from some source or other. This preparatory activity is introduced as a new intermediary which must first be dealt with before the animal can proceed from its original position to the attainment of the goal. This procedure can be described as the *fabrication of a tool*.

In experiment No. 7, the fruit was again placed out of reach in front of the cage, but neither a stick nor anything like a stick was at hand. In the rear of the cage, however, there was a dead tree, branches of which could easily be broken off and used as sticks.

In experiment No. 8, a swinging rope suspended from a horizontal bar for gymnastic purposes was slung over the bar three times in a manner easily comprehensible to a human being. In order to reach the goal this

rope had to be used, but first it had to be uncoiled from the bar.

Experiment No. 9, was still more difficult, for the rope had been removed from its hook, and lay upon the floor; before it could be used it had to be re-hung.

In experiment No. 10, a box that must be placed under the goal in order to reach the fruit was filled with stones so that it was too heavy to move until the stones had first been taken out.

In experiment No. 11, two bamboo sticks were provided, each of which was too short to reach the goal;

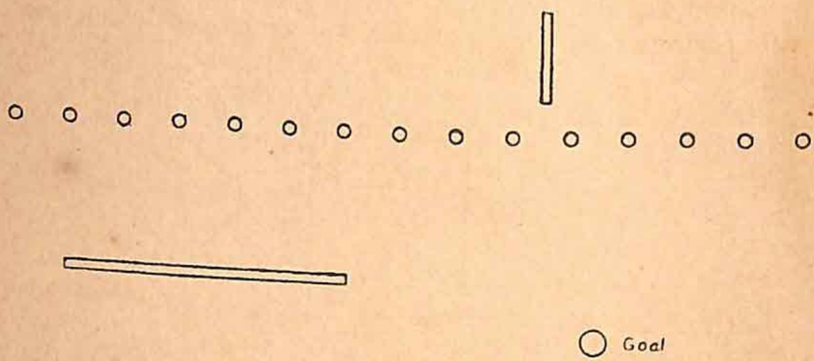


FIG. 10. [After Köhler.

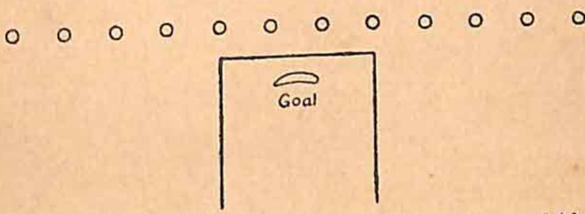
but one stick could be fitted into the other, and when thus put together the tool would be of adequate length.

In experiment No. 12, the "building" test, the goal was too high to be reached by a single box, but if two or three boxes were piled one upon another it might then be attained.

The indirect means to the goal were now elaborated. Before the original goal another goal was introduced which could not itself be directly attained. In experiment No. 13, the animal sat close to the bars of its cage, opposite the goal which was outside. In the animal's hands was a stick, which, however, was too short to reach the goal. Outside the bars, and some two metres to one side of the goal, but lying

nearer the bars, was placed a longer stick which could not be reached with the hand, but could be reached with the aid of the shorter stick (see Fig. 10). In experiment No. 14, the stick with which the goal could be reached was hung from the roof, and could be attained only with the aid of a box placed under it. This experiment could then be still further complicated by having the box filled with stones.

The principle of indirection was then varied in two ways. (1) By indirection in the use of the tool: Is the animal capable of finding an indirect means of employing the tool by which the goal is attained? In experiment No. 15, a device was employed which we



[*After Köhler*.

FIG. 11.

shall call a "detour-board." The animal sat near the bars, and at a distance of about forty-five centimetres from a square drawer, with open top and lacking a rear wall, which was placed before it outside the cage (see Fig. 11). The fruit was then put in this drawer near the side toward the animal. The animal received a long stick in his hand, but if the goal is to be attained, the fruit must first be pushed away from the animal, which is contrary to his usual method of bringing the food directly towards him. After the food had been pushed back until it was free from the drawer, it had to be pushed to one side; only after it was completely outside the drawer could it be brought forward. This detour involves an indirect procedure in the true sense of the word.

In experiment No. 16, a further complication was

introduced; in order to obtain the food, the stick had first to be removed from the place where it hung by an iron ring, six centimetres in diameter, upon a vertical iron rod, thirty-five centimetres long, which extended from a box. Before making use of the tool, the animal had to remove it from this rod, which meant that the animal must lift the ring in a direction at right angles to the direction of the goal.

A second variation was as follows: (2) In the course of using the tool, the goal was brought into such a

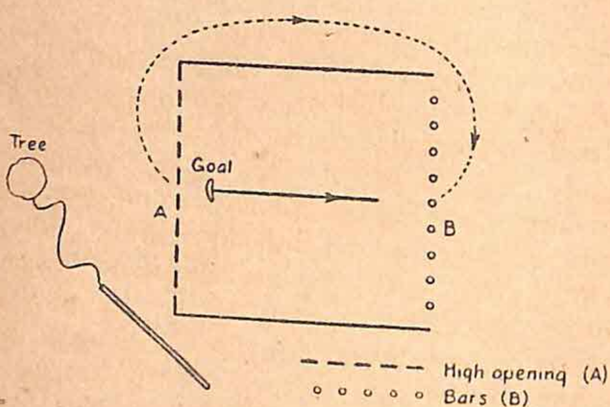


FIG. 12. [*After Köhler.*

position that it could be attained only through an alteration of the animal's position. In experiment No. 17 (Fig. 12), the fruit was placed near the side wall (A) of a large cage which was closed with horizontally nailed boards. One of the upper boards was removed so that the animal could reach inside the cage, though not far enough to touch the floor where the goal rested. The opposite side of this cage (B) was provided with bars through which the animal could also reach, though not far enough to attain the goal when it was placed near A. A stick was then provided which could only be used on side A, since it was fastened by a rope to a tree on that side. In order to secure the fruit, what the

animal had to do was, first to push the goal with the stick towards side B, and then proceed to the B-side of the cage, and procure the fruit by reaching with his hand through the bars.

The plan of all these experiments was to make them perspicuous to the animal. The later and more complicated tests presuppose that the simpler tests have already been successfully performed; thus new factors are systematically introduced, in order to make the solution constantly more difficult. By this method it was easy to find out from the failures that occurred what the animal's difficulties were.

The reader may ask if the chimpanzees were able to solve all these problems. Before answering, let us state that individual differences are so marked that one really ought never to speak of the capacity of a certain species. What one animal can do, another can not, and in all these experiments marked individual differences were both demonstrated and measured. With this reservation, the answer to the question is that only one of these experiments (No. 9), failed completely in the case of each animal tested. This was the test in which the rope had to be fastened to a hook in the roof. All the other problems were solved, and most of them as completely as one could wish, though in a few cases it was obvious that the limit of the chimpanzee's capacity had been reached.

By considering the concrete processes involved in the most important of these tests, we can give an account of the chief results. We shall begin with the experiments dealing with the employment of a tool. In this connection, No. 2 deserves detailed description. All the animals were able, without hesitation, to draw in the fruit when it was attached to a string, even when the string was very long; the test having been successful with a string as long as three metres. Nor was this task accomplished in the manner in which an animal might play with a string which it happened to

find on the ground, and thus by chance come into possession of the fruit attached to the end of it; on the contrary, it was observed that the string was always drawn "with an eye on the objective." Glancing toward the goal, the animal would begin to draw the string; the animal's behaviour being always directed upon the goal rather than upon the string. We might imagine this to be an obvious procedure for any animal, but when Köhler made a comparative test with a dog that had shown considerable capacity in other experiments involving ordinary features of indirection, he found the dog quite unable to carry out this act. Although taking the liveliest interest in the goal, the dog never took any notice of the string beneath his nose.[196]

This experiment was also varied with the chimpanzees, so that, besides the actual connection of one string with the fruit, other strings were also placed near by, all leading in the direction of the goal. In this test it appeared that any string extending to the fruit might be pulled, whether it was fastened to the fruit or not, but that among a number of strings it was the shortest rather than the right one which was the more likely to be grasped. It would seem that visual factors determine the behaviour of the chimpanzees in these simple tests, and that a visual connection may take the place of an actual connection—as when a noticeable visual characteristic, such as the shortest length, determines the choice from among a number of strings of different length.[197]

Regarding the employment of sticks in experiment No. 3, attention may be called to the following details. This problem was also mastered by all the animals. Some animals, indeed, were already familiar with it when the experiments were begun. With others, where the experiment called for the use of a stick for the first time, it was observed that from the very start the animal would place the stick correctly behind the

goal in order to fetch the fruit forward. The employment of sticks can again be made more difficult by a simple alteration of the experimental conditions. The farther away the stick lies from the critical position, the more difficult it is for the animal to make use of it. It sometimes happens that sticks which the animals have previously used lose their significance when they are removed to a sufficient distance. If a stick is so placed that it is not visible when the animal's gaze is on the goal, or in the course of a wandering glance which is limited to the region of the goal, its employment may be prevented. Even if the animal occasionally looks at the stick, it does not necessarily employ the tool, because it can not see both the stick and the goal at the same time. In this respect, one might say that the chance of a stick becoming a tool is a function of a geometrical constellation. This limitation, however, holds only at the outset; for animals that have often been placed in such a situation soon overcome this difficulty, and thereafter the solution is no longer hindered by a visual separation of the goal and stick.[198]

From this significant influence of visual factors we can understand the actual accomplishment of the animal in his employment of the stick; it is not merely a matter of seeing or noticing an object such as a stick, because before the object is employed it must cease to be an isolated neutral thing to the animal, and become a member of the situation at hand. The object, must, in short, become a " tool." As a necessary condition for correct behaviour of this kind, an *alteration must occur in the object of perception*. What at the beginning possessed only the character of "indifference," or "something to bite upon," now obtains the character of a " thing to fetch fruit with." It is thus easy to understand how a spatial separation of the stick and the fruit might render this process difficult; because an isolated thing can spring into a complex more readily when it

can be viewed simultaneously with the complex than when it is spatially remote from it.

The difference between the behaviour of dog and chimpanzee in experiment No. 2 indicates that for the chimpanzee the string at once belongs to the complex of the goal, whereas for the dog the string remained an isolated object which never entered into this complex at all.

The act of employing a stick seems to involve a transformation in the situation confronting the animal; for the stick, which at first was an object of indifference to the animal, now becomes definitely related to the situation. What the animal has actually learned is to make an irrelevant object relevant to the situation, which is something quite different from an external connection between a certain stick in the field of perception, and a certain sequence of movements. If, for instance, a stick is not available in a situation that requires its use, something else may be employed, such as a piece of wire, the rim of an old straw hat, or a wisp of straw. In short, under these conditions, "all objects, especially of a long or oval shape, such as appear to be movable, become 'sticks' in the purely functional sense of 'grasping-tools.'"[199] Indeed, one of Köhler's apes fetched its coverlet from its sleeping-room, and, pushing it through the bars, was able thereby to whip the fruit within reach.

These performances, like those referred to above (p. 187), also indicate transfer, and from the instances here described it may confidently be said that transfer can *not* be explained in the manner suggested by Thorndike. The chimpanzee's perception of the situation is by no means so obscure that, in a purely visual sense, either a handful of straw, or indeed a coverlet—which furthermore had to be fetched from another room, and did not originally belong to the situation at all—is identical with the stick which was first employed, or so like it that the animal can not apprehend a difference. On the contrary, only one conception of the performance

is possible—that the animal has acquired an ability to introduce "tools" into certain situations. Nor is this ability limited to the particular thing with which it was acquired; on the contrary, it is an acquisition of a much more general nature. As Köhler expresses it, the stick as it appears in the field of vision has acquired a definite *functional value* in certain situations, and this effect is itself carried over to any object which may have certain general characteristics in common with sticks, even though these objects have a very different appearance. What is going on in the phenomenal world of the chimpanzee's mind is made concrete to us by one of Köhler's observations. In watching an animal tantalized by the fruit which he can not reach until he has hit upon the employment of a stick or some other tool, Köhler remarks that "even before the chimpanzee has happened on the use of sticks, etc., one expects him to do so. When he is occupied energetically, but, so far, without success, in overcoming the critical distance, anxiety causes one's view of the field of action to suffer a phenomenological change. Long-shaped and movable objects are no longer beheld with strict and static impartiality, but always with a 'vector' or a 'drive' towards the critical point." [200]

A transfer of learning from one thing to another results, therefore, from the sensible application of a certain principle of configuration or *Gestalt*. First, sticks, and later other things, come to acquire a place in the situation, and to enter into its configuration as members. The implication previously suggested (above p. 190)—contrary to Thorndike—with reference to primitive modes of transfer, attains a greater degree of probability in the light of these considerations; and this behaviour signifies something more than a mere matter of attention. Bühler, however, seems to think that these cases of transfer in Köhler's animals can be explained by attention alone. Whenever we seek an object, according to Bühler, a dispositional readiness

to make observations is aroused in us. Accordingly, if the ape sits "near the bars of the cage while an attractive morsel lies outside, the well-known act of fetching it with a branch is the idea which occupies the ape's mind; and however vague this idea may be, if the ape chances to be moved by a restlessness to run about the cage—the goal always before him—sticks with which the fruit can be drawn will be the things which most readily emerge in his consciousness."[201] Bühler regards this explanation as in agreement with Köhler's idea of a functional value, but I must confess that what seems to me the most important thing about Köhler's hypothesis is altogether lacking in Bühler's explanation. To say that "things with which one can draw fruit" emerge in the animal's mind misses the point. By an act of attention a stick or a coverlet may come into the focus of attention; but these articles remain what they were—a stick of wood, and something to sleep under. Although they may also be "tools" suitable for drawing fruit, no mere act of attention can endow them with this property. Attention, for which "seeking" is a natural condition, is only a secondary consequence of this process. The situation is unsolved, and presses for solution, and the animal's behaviour consists in transferring to certain things the characteristics of a "tool" which they did not previously possess.

A task of this kind may seem quite simple; but when we compare the performances of these chimpanzees with those of monkeys in similar situations, we find that they are really high-grade achievements. In the experiments of Buijtendijk, and also in those of Nellmann and Trendelenburg, it was found that monkeys will not employ a stick or a rake in order to attain fruit, unless the tool is placed in a proper position as, for instance, the rake already behind the fruit and its handle within easy reach. If the tool is less favourably situated it is never used. No monkey would ever place the rake properly behind the fruit.[202]

Experiment No. 4, involving the employment of a box in attaining a goal which hung too high to be otherwise reached, throws some light upon the nature of the performance when it does not at once go smoothly. The animal in this case was Koko, the youngest ape at the station. At first he sprang and struck at the goal, and then he went away from the wall upon which it was hung, but only to return again. "After some time, on turning away from the wall, his eye fell on the box: he approached it, *looked straight towards the objective,* and gave the box a slight push, which did not, however, move it; his movements had grown much slower; he left the box, took a few paces away from it, but at once returned, and pushed it again and *again with his eyes on the objective,* but quite gently, and not as though he really intended to alter its position." [203] The procedure was repeated, however, and the next time the box moved some ten centimetres towards the goal. The goal was then made more attractive by the addition of a piece of orange. A few moments later Koko again stood by the box, grasped it suddenly, and pushed it in one movement almost exactly under the goal. He then climbed upon the box, and snatched the fruit from the wall. "Enriching" the goal had sufficiently strengthened the animal's impulse to bring about the solution. One can not say that the animal was at first too indolent to employ a solution he already knew; for a few moments later the experiment was repeated with the goal hung upon another part of the wall, some three metres distant from its former position, and this time the animal failed altogether. The first weak pushes which Koko gave to the box must therefore be regarded as steps preparatory to the true solution. Thus, although the box tended at once to enter into the situation, it was not evident at first how its entrance should be effected. "A single word is appropriate in describing the animal's behaviour during this period, namely, that the connection between

the box and the goal was beginning to 'dawn' upon him.")

The ape was unable to repeat the solution; for though tested again on the same and on the next day, and later on four different days with greater intervening periods, each trial was unsuccessful. On one occasion he placed the box so near to the wall that standing upon it he could almost have reached the goal. He did, indeed, climb upon the box at once, and reach as far as he could, but he made no attempt to move the box. Obviously it is not enough to bring the box into a general relationship with the situation; for a second point of importance is *how* this relationship shall function.

The experimenter was forced to interrupt these unsuccessful experiments, because the ape in his exasperation would end by rudely mishandling the box. After a pause of nine days—nineteen days after the first experiment—the test was renewed. This time the solution was fairly prompt, and could be repeated thereafter without hesitation. In the meantime the only noticeable after-effect of the first solution was equivalent to the words: "There's something about this box!" [204]

We have described this experiment in full because it furnishes some insight into the stage that intervenes between perplexity and a complete solution. The experiment shows how the direction of the solution was prepared for before the first success was achieved, and how thereafter all that remained was a kind of "place-analysis" which reminds us of Ruger's experiments (see above p. 195).

From another observation upon the employment of boxes, in a later and more complicated experiment, we can see what this behaviour involved. If an animal is unable to make use of a solution with which it is already familiar, the conditions that interfere often indicate what are the most characteristic features of the act.

For instance, one of the animals, Chica, strove with all her might to attain a goal suspended from the roof, without ever using a box which stood in the middle of the room, although she had already mastered the use of boxes in similar tests. It could not be said that the box was overlooked, for the animal repeatedly squatted upon it when she was out of breath, and yet she made not the slightest effort to bring the box under the goal. During the whole time, however, Tercera, another ape, was lying on the box; when at length Tercera chanced to fall off the box, Chica grasped it immediately, carried it under the goal, and, mounting it, snatched down the fruit.[205] From this behaviour it may be inferred that the box upon which Tercera was lying was not an "object from which to reach fruit," but "something upon which to lie." Consequently the box simply did not come into connection with the goal so long as it possessed a definite configuration of its own that made it inappropriate as a tool in another situation. To release a thing from one configuration, and transfer it by reconstruction into another configuration, would seem to be a relatively high-grade accomplishment. Nor is this difficulty confined to chimpanzees; on the contrary, it plays an important part in human thought. For instance, when you have need of a shallow dish, it might never occur to you that you could use the cover of a pot, unless such a cover happened to be lying before you on the table, away from the pot, in which case probably you would at once make use of it.

From the point of view of an adult human being, it is not easy to judge whether the problem of setting aside obstacles is simple or difficult for an animal. To us, the obstacle-experiment, No. 6, would seem to be far easier than the application of a stick, or a box, as a tool. To a chimpanzee, however, the solution of No. 6 is rather more difficult; for not all the animals were able to accomplish it unaided. In general, a chimpanzee is able to fetch a tool from a considerable

distance and bring it to bear upon a situation more readily than he can remove even a very simple obstacle from the same situation ; the reason being that it is always hard to break up a definite configuration already existing.[206]

In the fabrication of tools we find examples in which a "reconstruction" of the situation was successfully carried out. In experiment No. 7, for instance, the achievement consists in seeing a branch as separate from the tree of which it is a part ; that is to say, a thing which appears as a branch must be seen as a stick, and this proved to be a very difficult task for the less talented animals. It was noticed also that before a dead branch would be broken from a tree, the animal first tried to release a bar from its cage, because the bar was visibly a more independent object than the branch.

Experiment No. 8 indicates a new difficulty, and consequently a new aspect of the achievement. After experiment No. 5 had been successfully carried out, the test was repeated under the conditions of experiment No. 8 ; and the result was that every animal strove to pull the rope down from the bar into the normal position from which it could be used as a swing. Yet not a single animal solved the problem correctly by first uncoiling the rope. What the animals did was to grasp the rope anyway, and pull it down as far as it would come. Only once in a while could the rope then be used to swing with, and only the best of the gymnasts could employ it successfully. The nature of this behaviour with respect to a coiled rope leads one to think that apes see these simple orderly coils, not as we do, but rather as a confusion of strands like a snarl ; and we, too, are apt to attack a snarl without any definite plan, by grasping a strand at random and pulling at it. Though objectively a simple construction, a coil of rope seems to be something that a chimpanzee is incapable of apprehending as a clear-cut visual form ; instead, it seems to appear to him as a more or less chaotic figure,

and this indicates a certain limit to his capacity of achievement. This limit, however, can be passed; for two years later, when two of the same animals were again confronted with this problem, one of them, Chica, solved it at once, completely and adequately, by uncoiling the rope as well as any man could. The other animal, Rana, although less successful, at least behaved with much greater assurance than she did before. Some development in the capacity of visual articulation and reconstruction seems therefore to have taken place in both these apes, although Köhler estimates the degree in which this capacity can be improved very slight.

R/ Unusually impressive was experiment No. 11 with two sticks that could be fitted together. It was the cleverest animal, Sultan, who was here tested, and even he depended for his success upon the aid of chance. For over an hour Sultan had laboured in vain, trying among other things the following procedure. One stick was first stretched as far as possible in the direction of the fruit, and then carefully pushed still farther by the second stick until the goal was actually touched. Thus, a contact was made with the goal, but unfortunately one that could not be used. Useless though it was, this procedure represents a genuine solution in so far as it produces a unitary configuration connecting the animal with the fruit. The experiment was then given up, and Köhler departed. Sultan, however, retained the two bamboo sticks, and the keeper remained at his post. It was the keeper who observed the animal, first sitting upon a box which stood near the bars, then rising, picking up the sticks, and after reseating himself on the box, beginning to play with them aimlessly. "While doing this, it happens that he finds himself holding one rod in either hand in such a way that they lie in a straight line; he pushes the thinner one a little way into the opening of the thicker, jumps up and is already on the run towards the railings, to which he has up to now half turned his back, and

begins to draw a banana towards him with the double stick. I call the master: meanwhile, one of the animal's rods has fallen out of the other, as he has pushed one of them only a little way into the other; whereupon he connects them again." So runs the report of the keeper, and Köhler himself arrived in time to see the rest of the animal's performance of refitting the sticks together and securing the fruit.[207] After his first success Sultan repeated the act a number of times, without stopping to eat until he had drawn into his cage, not only all of the fruit, but also a number of other things. He appeared to enjoy the act, and he retained the method of solution so well that on the following day he was able to construct a still longer stick from three bamboo stalks. Although Sultan's solution depended upon the help of chance, chance operates here quite otherwise than it does in the experiments described by Thorndike; for it was not chance that led to the goal, nor did chance provide a practicable tool; instead it was the chance-situation when the two sticks were in line with each other, that favoured the correct solution. The solution itself was authentic, as the animal's subsequent behaviour proves. As soon as the two bamboo stalks were seen as one, they were likewise seen as the tool which had previously been lacking. Although a "fortunate variation" assisted in the solution, the solution itself is not to be counted as having been one of chance. In order rightly to evaluate the assistance thus rendered, we must think of ourselves in a similar predicament. While it is, of course, a greater accomplishment to be able to solve a problem by thinking it out, it is often difficult enough for human adults to make use of a chance circumstance as Sultan did when he passed from a type of behaviour without insight to an action which possessed this quality. Thus chance and insight are by no means opposed, for insight frequently comes through the employment of chance.

Acts of building, as described in experiment No. 12, again furnish new data; for in these performances the behaviour of the animals indicated very clearly that two different problems were involved. The one of setting a box on top of another box is no great task for an animal which already knows how to use boxes; but the problem "of placing one box upon another so that it will stay there is extremely difficult"; this problem requires that a body of a certain form shall be united with another of similar form to produce a definite result —something which the chimpanzee never accomplished with insight, but only by trial and error. An ape will use structures so insecure that we would scarcely dare touch them with our finger for fear they might topple over; yet the ape does not hesitate to mount them, and with his great bodily skill he is often successful in reaching the goal before the whole building collapses. Here again we have evidence that the animal's visual insight is limited.[208]

These experiments in building are important from still another angle. The building or stacking of boxes was never itself performed with insight, but remained always at the level of "trial and error." It ought therefore to show the effects which are supposed to be a characteristic of learning by repetition. Köhler's apes loved to stack boxes, and they practised this art assiduously under his observation for two years. Yet during all this time their structures showed no improvement, and were just as flimsy at the end as they had been at the beginning. Some psychologists believe and teach that every achievement which is not instinctive is a result of learning by "trial and error." Perhaps this negative result in the case of chimpanzees will make them a little more cautious in applying this principle of learning as broadly as they sometimes do.

Experiment No. 13 was solved with insight by the more talented animals. What was demanded in this solution is again shown by the behaviour of the less

talented apes, who, being unable to detach the shorter stick from the configuration involving bars and goal, could not compass the more complicated configurations leading from the shorter stick to the longer stick, and from this to the final goal.

Where problems requiring detours that involve independent, intermediate ends are correctly solved, the primary and secondary goals belong in such wise to the total configuration that they acquire very different values for the animal. This is again revealed by characteristic errors. In experiment No. 14 Koko was moving a box to the wall upon which hung a stick needed to secure the fruit; but on his way he had to pass by the fruit, and when he came near it "he was suddenly deflected from his straight course towards the stick, and began to use the box as if it were a stick with which to reach the fruit." Thus, influenced by the greater effectiveness of the fruit itself, when it was near at hand, the originally present and correct configuration of the solution was quite destroyed before it could be carried out.

The "detour-board" of experiment No. 15, in which the fruit had first to be pushed away from the animal before it could be retrieved, was in many respects informing. In the first place this problem was so extraordinarily difficult that even Sultan was unable to solve it completely. Only one animal, Nueva, after poking vainly at the fruit a great many times, suddenly reacted with the correct solution by shoving the fruit to the open end of the drawer—that is, at an angle of 180° away from herself. But even in her case, when the goal was almost at the open end of the box, a counter-action was suddenly made which brought the fruit back some five centimetres towards her, after which the problem was correctly solved. Counter-actions of this sort appeared again and again in later experiments, showing how difficult it was to overcome a strong contradictory tendency. And yet one would be inclined

to think that, for an animal that makes detours so easily and so naturally as the ape, this one with the aid of a tool ought to be a very simple matter. That it is not, forces us to realize that " even intelligent behaviour, the achievement of insight, will not submit to ' intellectualistic interpretation.' "[209]

Sultan was the only other animal to succeed in this test; and he was aided by chance as he had been in the case previously described of splicing sticks.[210] He was then able to accomplish the task of first pushing the goal at an angle of 180° straight away from himself; but the experiment had to be made easier for all the other animals. This was readily done by simply turning the drawer at a certain angle; whereby one could also measure the degree in which the test was made more easy, because as the detour-angle became smaller, it was found that animals hitherto unable to accomplish the task could now do so. Thus the angle at which the problem was first solved could be taken as a direct measure of the accomplishment, and likewise of the intelligence employed. When the drawer lay parallel to the bars of the cage, so that the detour-angle was 90°, all the animals were successful. The rank attained by the animals in these tests also corresponded exactly with one which Köhler had previously estimated. Thus the detour-board proved to be an excellent test of intelligence.

Test No. 16 also indicated a limit in the ape's intelligence. To lift a ring from a nail was a performance which only the cleverest animals could carry out, and then only in their best moments. It was not merely by chance, however, but with insight, that the task was then accomplished. " The ring over the bar (the nail) seems to represent to the chimpanzee an optical complex which may still be mastered completely, if the conditions are for the moment favourable, if there is concentration of attention and so on; but it has a strong tendency to be seen less clearly if

the animal fails to make the proper effort on its part." [211]

Considering all these tests together, we find the animals actually solving new problems as these are presented to them. Furthermore, the essential thing about these solutions is not a new combination of movements with which the animals were already familiar, but a "new organization of the whole field." As has been shown in the previous discussion, the assumption that we are dealing with new combinations of old modes of behaviour can be maintained only if we accept chance as the creator of these new combinations. For one who understands Köhler's experiments, it is impossible to assume that chance played any such part in them. This conclusion is obvious when we review the two chief arguments advanced by Thorndike in favour of the chance-hypothesis. Thorndike's first argument, derived from the form of the time-curves of his cats and dogs, is inapplicable; for in view of the long periods of time which often intervened in Köhler's experiments before the animal found a solution to the problem, it is quite apparent that time-measurements of the chimpanzee's behaviour would not decide the question of chance-insight. These periods were always occupied, either by activities which had nothing whatever to do with the solution, or else by rest. During such a pause, however, Sultan would scratch his head slowly, otherwise moving nothing but his eyes or perhaps his head, while he surveyed the situation about him in the most careful manner; [212] which indicates clearly enough to the observer in what kind of behaviour the ape was engaged during these vacant periods. As for the solution itself, it occurred typically as a single course of action without a break; and when the test was repeated, the correct action would be almost immediately carried out. Thus, if one were to use time-curves at all, their evidence would be very strikingly against the operation of chance.

In discussing his own experiments with monkeys, Thorndike also notes the inconclusive evidence of his time-curves. From his remarks, however, it would appear that the time consumed in learning is more important as a measure of achievement in lower apes than it is in the case of anthropoids. "In the long run," he says, "the times taken do represent fairly well the amount of effort."[213] Nevertheless, these same time-curves of Thorndike's monkeys are more indicative of sudden achievement than they are of a slow and gradual process of learning.

Thorndike's second argument based upon "stupid" errors appears to fit our case as little as the other. Altogether Köhler observed but eight instances which might be called "stupid" errors. These are described as "*after-effects* of former genuine solutions, which were often repeated, and so developed a tendency to reappear *secondarily* in later experiments, without much consideration for the special situation. The preceding conditions for such mistakes seem to be drowsiness, exhaustion, colds, or even excitement."[214]

Along with these "bad" errors, certain other mistakes occurred which have a special significance for the behaviour involved. These other errors arise when one part of the principle of solution is correctly understood, while at the same time the problem involves some difficulty with which the animal is unable to cope. Thus, for instance, in order to increase the length of his stick, the animal would often seize two sticks and place them with the end of one touching the end of the other. This provided him with a longer stick, to be sure, but not with one he could use. It was a similar procedure that furnished the initial stage of Sultan's double-stick solution (see above, p. 217). To give still another example, the following behaviour was observed in the building-experiment. Chica found that with one box alone she could not attain the goal, no matter how high she jumped from it. "Suddenly

she seizes the box with both hands, holds it by a great effort as high as her head, and now presses it to the wall of the room, close to which the objective hangs. If the box would 'stick' to the wall, the problem would be solved; for Chica could easily climb up and reach the goal by standing on it." [215] "Good" errors of this kind certainly can not be explained by chance; because the acts we have called "good" errors do not appear in arbitrary situations, but only under conditions where they signify something "good,"—that is, where they actually bring the animal somehow nearer its goal.

With respect to the problem of achievement considered at the beginning of this chapter, Köhler's experiments show that chimpanzees accommodate themselves to new situations, and solve new problems, by actually undertaking new modes of behaviour. As Köhler has expressed it,[216] "the directions, curves, etc., of these solutions could arise autonomously (not necessarily "from experience") in the static situation." This conception also supports the explanation given above of the animal experiments reported by American psychologists. In the achievements of Köhler's chimpanzees we find new creations of a pure type occurring, in these experiments, quite free from chance. Instead of the solution first arising by chance, and thereafter becoming more or less "understood," understanding, or an appropriate transformation of the field, precedes the objective solution. We may therefore be permitted to call solutions of this kind intelligent performances of a primitive order. When a solution is found, the situation is altered for the animal in such a way that a gap in the situation is closed; that is to say, the desired but unattainable fruit has come within reach. We have here the characteristics of "closure" already met with in a previous connection (above p. 109); for when the problem is "solved" everything in the perceptual situation depends upon the total

configuration. Likewise every movement has its place, so that the configuration, as we have called it, becomes unequivocally defined and complete. Dynamically considered, a configuration in time—and we now understand by configuration, not merely a plan in the field of perception, but the total process of the solution leading up to the attainment of the goal—has both a *beginning* and an *end*.

We might try to describe the creative process of learning a little more definitely. What is the nature of the change which takes place in the phenomenal field when a problem is solved, or a goal attained? Broadly speaking, there seem to be three kinds of transformation which may be referred to as *unification*, *analysis*, and *articulation*. For instance, unification occurs in Experiment 11, when two short sticks become parts of a single long stick. At first the phenomenal field contained two sticks, *one plus one*; finally it contained *one stick with two parts*. A similar description is applicable to Experiments 3 and 4, involving the simple use of a stick or box. In these cases the stick or box, which were at first extraneous to the chief configuration of the ape's phenomenal field—namely, the longed-for but unattainable fruit—become parts of this configuration.

Analysis in turn, is characteristic of Experiments 6 and 7, which require, respectively, the removal of an obstacle, and the employment of a branch as a stick. A box placed against the bars of the cage is one with the cage. Even more strongly, the branch is a natural member of the tree. In both cases the solution of the problem requires that what was *one* must become *two*. An analysis of the same kind is involved in Experiment 13, where the stick in hand, which is too short to reach the fruit, is nevertheless so closely knit to the objective, that a special effort is required to break this tie before the short stick can be used to draw in the longer stick which will reach the fruit.

Experiment 8, in which a rope must be uncoiled, is a test of articulation, as are also some of the modifications introduced in Experiment 2 when more than one string leads from the objective to the animal. The lack of a sufficient capacity of articulation is revealed in the failure of the apes to improve their performance in stacking boxes. The ring on a nail in Experiment 16, and the performance with the detour-board (Experiment 15) are also instances of articulation. Still another form of articulation is required when the roundabout way to the objective includes primary and secondary goals, as it does in Experiment 14. The error committed by Koko in this test (see above, p. 220) was a failure to make the correct articulation of the part-problem upon which he was engaged.

While our classification is not a full description of these performances, it is helpful in understanding what takes place in the creative process of learning. All three classes are generic, and each of them includes specific differences which must be studied in detail. Thus, the examples given under each of the three headings differ from one another in certain important respects. Furthermore, these three types of process are by no means independent of one another. In many concrete acts they will all occur conjointly; a transformation of one kind will often presuppose a transformation of some other kind. Of the three processes, that of articulation seems to be the most fundamental. Without articulation there can be no analysis; because a whole can be broken only along lines of cleavage which are determined by its internal contours. Whenever a whole is entirely diffuse it is unanalyzable. Unification also presupposes articulation; because the field must contain articulate parts before these parts can be unified in a single whole.

Finally, it should be noted that articulation refers, not only to the case in which an inarticulate or a weakly articulated whole becomes more articulate, but

also to what we might call *re-articulation*. Here we have a redistribution of weight, because the forces that obtain between different parts of the whole have altered their direction. Thus we might say that two sticks articulate when Sultan places them end on end, and that they are re-articulated when he splices them together. The forces obtaining between the two sticks constitute at first merely an added length; they constitute a direction towards the objective as soon as they stick together.[217]

These last considerations imply a general conclusion as to the nature of learning, which is of great theoretical importance, and has a direct bearing on Educational Psychology. *Learning is never entirely specific.* If an organism has intelligently mastered a problem, it has not only learned to master the same problem when it occurs again, it has also become capable of solving different problems which previously it could not solve. Thus learning is a true development, and not a mechanical addition of performances.

§ 7—*Criticism of Köhler's Experiments*

It was hardly to be expected that the new theory of learning which has grown out of Köhler's experiments would be accepted without adverse criticism.[218] Bühler,[219] Lindworsky,[220] and Selz,[221] all ask: Do these experiments really prove that animals possess insight; and, if so, what kind of insight? Köhler's proof of insight rests on the fact that his apes directed their behaviour by means of the relations existing between the objects in their environment. To Bühler, this proof is inadequate. As an alternative hypothesis he maintains that the simple act of "noting" a relation is no better evidence of insight than the act of "noting" sensory contents.

In the second edition of his book on Mental Development,[222] Bühler writes that the noting of relations

might be a considerable achievement without indicating any insight on the part of the animal—a distinction which he thinks of importance.

In his fourth edition, however, Bühler omits this explanation. Instead, we find the modified statement: "If we assume that chimpanzees have 'transitional experiences' which they note, why should not this noting explain their behaviour? This, indeed, would be a considerable achievement," etc.[223] In his second edition Bühler had stated that, "in the opinion of many psychologists, relations, like sensory contents, are capable of being directly observed or 'noted' without a true experience of judgment (cf. Stumpf, Selz, and others)." The modification in his later statement also stresses the importance of "noting," and it is chiefly against the employment of this concept that the following discussion is directed. The problem of "transitional experiences" will be discussed later.

Let us ask what is implied by the statement that material relations are "noted"? In the first place, the statement refers only to the overt behaviour of the animal—for instance, that a connection between the stick and the fruit such as might be readily grasped by a human observer has likewise been grasped by the animal. But "noting" is also commonly used as a description of inner behaviour; Bühler himself also employs the term in this way. The usage would signify that, along with the phenomenon in which this possible connection has been presented to the animal, something else is added—the "noting" of it—which may or may not be represented phenomenally; and hence the previously unnoticed becomes a noticed phenomenon, though it remains otherwise unaltered. A certain blue within the field of vision does not become another colour-tone through my noting it; and, according to Bühler's argument, this assertion of persistence without alteration applies, quite generally, to all relations.[224]

This hypothesis can be attacked from two sides. In the first place, one can say [225] that a psychological description must, initially, limit itself to the determination of what is phenomenally given; whereas the employment of a concept like "noting" carries us beyond the phenomenal data of observation. When I say that I have failed to notice the difference between two colours, my statement is ambiguous and incomplete to a psychologist, because what he wishes to know is what I actually did note—what positive phenomena were present. In the case we have cited the answer may at least be twofold: (*a*) "I did not notice the difference, because, before my attention was drawn to the two colours, they looked identical to me," or (*b*) "I know nothing except that I did not notice any difference."

In case (*a*) the psychological description (without noting) would then contradict the first description (with noting); because it has been assumed that the noting of a phenomenon, whether of a sensory content or of a relation, leaves it qualitatively unchanged; whereas, here, in noting the phenomenon a relation of equality has been changed to one of inequality. At least I do not see how one can state the case otherwise from the point of view of Bühler's argument. Case (*b*) is the one for which the hypothesis of noting was primarily invented. But have we any right to substitute for an "unnoted" part of the visual field something which is noted for the first time after we have changed the direction of our attention? This substitution, however, is just what Bühler's hypothesis implies, leading to the tautology that we do not notice the difference because we do not notice it, although it is all the time phenomenally present.

A real explanation will exclude the construction which we have just characterized. There is no evidence that the part of the field is the same after it is noticed that it was before it was noticed. Now the question arises: What was the nature of this field in which an

objective difference was overlooked? An answer to this question is not very difficult. The phenomena which now appear as two colours were previously not there at all, even though their stimuli were operative. These stimuli would then provoke phenomena having the characteristics of what we previously called a *background*. Noting would then signify that a background-effect has been transformed into a figural effect. If we apply this interpretation to the experiment with the apes, the implication is that, when the ape notices certain material relations, he transforms the perceptual field in which these "material relations" were previously lacking into one in which they become central. Thus the field is altered so as to present a *new* configuration adequate to the problem. This, however, is what we have repeatedly referred to as the essential feature of the animal's achievement.

The hypothesis of noting can also be tested in a second way, by asking what it means in the explanation of a concrete case. Bühler assumes that the animals simply note the relations between objects as they are given; but the relations in which the parts of a situation stand to one another, and to the situation as a whole, are innumerable. A stick, for instance, may be at the right of an animal and at the left of a tree; it may be nearer to the tree than it is to the bars of the cage; it may be longer than a piece of wire which is closer at hand; and so on indefinitely. The theory must explain why, among all these innumerable relations, it is precisely the most important one that comes to be noted as the determinant of behaviour. What we should say is that a meaningful construction of the field with respect to the goal has taken place; and that the solution of the problem is nothing else than the arousal of this construction. Accordingly, the problem of innumerable relations does not exist for us, because relations as such do not determine a meaningful configuration. If one wishes to eliminate meaning, and

refer the event to the effect of a blind associative mechanism, one must be prepared to explain why it is precisely the significant rather than the insignificant relations that are noticed. It seems to me that Bühler has confused the issue by approaching it with a fixed definition of " insight," which presupposes a judgment involving experiences of certainty and assurance.[226] Since judgments of this sort have not been demonstrated in the case of chimpanzees, insight must therefore be denied them. But even if Bühler's description were appropriate to the behaviour of adult human beings when they act with insight, it would not at all follow that the simplest kind of behaviour with insight must likewise possess these characteristics of judgment. Thus Lindworsky, who has gone much further than Bühler in his criticism of Köhler's work, remarks that the apprehension of relations need not imply any assurance; this apprehension being " neither certain nor uncertain, but simply undoubted." We should say that "meaning" resides in the configuration, in the "significant relations" themselves, whereas Bühler seems to assume that in order to endow a content with meaning something new must be added. When we have thought it through, Bühler's criticism only leads us back to our own hypothesis.[227]

We shall continue this discussion by taking up another matter upon which Köhler has been attacked. Let us recall the experiments on choice-training which indicated the primitive nature of configural functions. In these experiments a test was made to find out if training was a matter of an association established between a movement and an " absolute " sensory content, or if the bearing of one upon the other—that is, the configuration itself—determined the animal's behaviour. The connection between the achievement and the configuration is certainly quite arbitrary and meaningless when food is placed on either the brighter or the darker paper, according to the will of the

experimenter. It should also be remembered that in the "critical test" which followed after training, both papers had food on them. Under these conditions either the "absolute" or the "configurative" choice might have been expected, and either would have satisfied the animal's want. Since from our point of view a test of "intelligence" involves a configural function, it ought to be possible to place the animal under conditions where there would be an alternative between an "absolute" and a "configurative" choice, the "configurative" response being intelligent, whereas the "absolute" response would remain unintelligent. An experiment of this kind was developed from the stick-splicing test (cf. above pp. 217 f.).[228] In connection with this experiment it should be noted in advance that in fitting the sticks together the ape always placed the thinner one within the thicker one, holding the thicker one passively in the left hand and moving the thinner one towards it with the more skilful right hand. In a special series of experiments Köhler employed four tubes of different diameters, so that No. 1 fitted into No. 2, No. 2 into No. 3, and No. 3 into No. 4. Two of these tubes, chosen in this serial order, were laid horizontally and parallel before the animal, sometimes the thinner and sometimes the thicker being nearer at hand. No. 2 now became the thicker when paired with No. 1, while it was the thinner when paired with No. 3. Out of twelve trials, Sultan grasped the thinner tube at once with the right hand, and the thicker with the left hand, eight times. In the other four trials, in which the sticks were at first grasped differently, they were changed "as quickly as they were seen in the hands, without any testing, and always before the animal undertook the performance itself." In the majority of cases the animal picked up stick No. 2 with the left hand, or with the right hand, according as it was to be combined with No. 1 or No. 3. In other words, the ape handled the objects with reference to

the formal relations in which they stood to one another. Chica, who had previously acquired the double-stick procedure from Sultan, behaved similarly in these tests. Only once in the twelve trials was the thinner tube placed in the thicker one by the left hand.

In this behaviour Köhler finds a proof of insight into the solution of the problem, because the apprehension of the bearing of the two diameters upon each other determined with certainty the function of each tube. Thus Köhler contends that the manipulation of things with reference to their important material relations can be employed as a criterion of behaviour with insight—that is to say, of intelligence.

This conclusion is attacked by Lindworsky,[229] who tries to show that the achievement can be explained without assuming insight on the part of the animal. Omitting Lindworsky's other arguments, I shall confine myself to the question whether or not the recognition of the difference in diameter of these tubes is a sign of intelligence. If this recognition were based upon a true apprehension of relations, it would be an act of intelligence, even according to Lindworsky. But Lindworsky denies that an ape can apprehend relations in this way.

Here we meet with a criticism directed against Köhler's theory of configural functions. Similar objections to those of Lindworsky have also been advanced by Bühler, while Jaensch,[230] two years after Köhler's publication, has reported experiments made with hens, in which he employed a method resembling that of Köhler (pp. 154 f.). Jaensch, however, has given his results a theoretical interpretation which agrees with Lindworsky and Bühler, and is therefore, in principle, quite different from the interpretation given by Köhler. In view of the consequences drawn from Köhler's theory at the close of the last chapter, we must now consider this other hypothesis in some detail.

We saw that after an animal has been trained to differentiate two things, A and B, in a certain way—

say with reference to their brightness—so that B is always chosen, then if a test-experiment is made in which B and C are presented to the animal—C differing from B in the same way in which B differed from A—in the majority of cases not B but C will be chosen. The result of these experiments was explained, in agreement with Kōhler, by saying that the animal was not trained with respect to the absolute presence of B, but with reference to the *bearing* of A upon B; accordingly, the configural property which C possesses with respect to B remains the same as that which B possessed with respect to A. In other words, two colours adjacent to each other are not perceived as two independent things, but as having an inner connection which is at the same time a factor determining the special qualities of A and B themselves. This statement agrees with the description of the phenomena in question, because under similar conditions introspection finds the most characteristic feature of such an experience the "togetherness" rather than the separateness of the two colours.[231]

The negative side of this thesis—that "absolute" training is less effective than "configural" training—is admitted by all investigators; but the positive side, which regards the configural functions as very primitive processes, is denied, and another explanation advanced in its place.

Schumann was the first psychologist to observe certain unique phenomena which accompany the process of comparison. Thus in the successive comparison of two circles, or lines of different length, an extension or a shrinking appeared in the field of vision according as the eyes passed from the smaller to the larger or from the larger to the smaller object. If one employed brightness- instead of magnitude-differences, this accompanying effect consisted in a "transitional experience" of brightening or darkening. The hypothesis we are now discussing, based upon the results of these experiments, is that the animals were trained with

reference to these *transitional experiences.* "In being trained so that 'dark gray is forbidden,' while 'medium gray is allowed,' what the hen in truth learns is that it is allowed food whenever a transitional experience of 'brightening' occurs." [232] In the test-experiments the animal chooses with reference to "configuration" rather than "absolute colour," because the transitional experience from B to C is the same as it was from A to B. The main difference between this and Köhler's explanation is that Schumann's followers retain the concept of sensation, supplementing it, in order to bring it into accord with the results of the doctrine of comparison, by the addition of the new concept of the transitional experience. It is the same procedure we have so often noted; whenever any new facts reveal a defect in an explanation previously employed, instead of doubting the accuracy of the explanation—once it has become firmly rooted—something is merely added in an attempt to make it adequate to the new facts.

Let us consider this particular addition more closely. Transitional experiences are added to "sensations," but the sensations are left quite untouched. The connections between the sensations remain completely external, even though one can infer the relations between the absolute elements from the transitional experiences, as both Jaensch and Bühler do. Jaensch, to be sure, goes even further; for, employing an expression of Brunswig's, he declares that the transitional experience "hovers and reigns between the two objects, since it is a quality of neither of them." [233] What this may mean, concretely, and what inferences can be drawn from it, we are not told; yet here is a decisive point, because the doctrine of transitional experiences, in so far as it is actually distinct from the theory of configuration, can signify only that to the absolute experience of A and B a transitional experience T is added as a third content to the other two; from which it might be inferred that T can undergo definite associa-

tion just as well as A or B can. Indeed, the hypothesis is stated in just these terms by Lindworsky. But that T should ever " hover " between A and B, as Jaensch maintains, is something new; for then A—T—B becomes a unified whole, the nature of which is itself in need of explanation. Is it, indeed, anything else than the "bearing of one upon another," the " togetherness " of the two, which Köhler has remarked?

It may be objected, however, that transitional sensations are observable data. But what of that? Nothing follows, so far as I can see, except that this " bearing," this step from one member of a pair to another, has been observed under the unnatural conditions of a laboratory experiment. When, however, the transition takes place in a natural way, one can not " see " it; being intent upon finding " sensations," one " sees " only A and B, whereas transitional sensations emerge only under quite special conditions. But the question is wrongly put to begin with, because the description is psychologically incorrect. It is incorrect to maintain that nothing is given in a phenomenal pair of colours except one colour here and another colour there; it would be equally incorrect to describe the accompanying figure (see Fig. 13) as one vertical and one horizontal line. What we actually see in this figure is an angle, and in the case of a pair of colours what we see is a combination, a configuration, for which we require no transitional experience. Indeed, any transitional experience that we may have must always presuppose the existence of a configuration.[234]

Fig. 13.

The following difficulty also arises in connection with the doctrine of transitional experiences. These experiences are quite unknown to most persons, and it requires a " careful psychological analysis " to apprehend them. What right, then, have we to regard them

as being the essential constitutents of a comparison, or to go farther and attribute them to hens ?

In reply to this objection the advocates of the theory of transitional experiences reply that our judgments may be determined by sensory impressions which themselves are too weak to be noticed. In support of this hypothesis Jaensch refers to a well-known experiment upon the perception of depth. If you look through a tube with one eye at a thread, you can readily recognize its approach or withdrawal. The image on the retina alters its width with the movement of the thread—the image becoming wider when the thread is nearer, and narrower when the thread is more distant. If, following Hillebrand's procedure, we replace the thread with an object whose displacement produces no such change in the retinal image—as, for instance, the sharp edge of a screen extending into the field of view—even considerable displacements will be quite unnoticed. From this Jaensch concludes : " In the case of the thread, judgment can rest only upon a change in the magnitude of the retinal dimensions which accompanies the alteration of the thread's distance ; and although this change is too small to be directly noticed as a change of magnitude, still it must determine the judgment of distance. The same is true of transitional experiences which . . . in spite of the very slight impression they make upon us, may yet serve as a basis of judgment." [235] In this explanation we again find the inappropriate concept of " noticing." If we eliminate this concept the facts can be described as follows : A change in the breadth of the retinal image does not necessarily produce a change in the apparent breadth of the phenomenon ; for under certain conditions this change of retinal breadth may, instead, give rise to a phenomenal difference of *distance*. But the mediation of the judgment by a phenomenally unnoticed change of breadth is a mere hypothesis and, in addition, one that is in principle undemonstrable.[236] I can cite a quite analogous

case where even Jaensch will recognize the validity of our interpretation. The enlargement of a retinal image may have the general effect of making the corresponding object appear larger. As a rule, however, the phenomenal enlargement is not proportional to the actual enlargement, but lags somewhat behind it; consequently the object seems to project itself towards us, and become clearer and more striking. Perhaps the best example of this is afforded by looking through lenses, such as those of opera-glasses; for the objects seen alter their apparent magnitude and distance very little, whereas their clearness undergoes a very striking increase. Yet in this case an analysis of the phenomenon into unnoticeable components which influence our judgment is quite out of the question. Jaensch himself has made notable contributions to the study of similar phenomena without recourse to any hypothesis of unnoticed sensations. It follows that there is not the slightest occasion for introducing these hypotheses into the explanation of phenomenal magnitudes, and hence the argument for the necessary existence of transitional experiences falls to the ground.

We are led to a like conclusion when we examine the argument employed by Bühler. Transitional experiences, says Bühler, "are like hearing the overtones of a clang—it requires some practice before one can find these ordinarily neglected factors of experience." [237] So-called clang-analysis, or hearing out the partial tones of a clang, has often been advanced as a striking demonstration of the existence of unnoticed sensations; yet Köhler and Eberhardt have now shown that if one examines the facts precisely and without prejudice, such an interpretation is unwarranted,[238] because clang-analysis is an artificial production of certain tonal phenomena which occur only under certain additional conditions, notably by reason of a special direction of the attention, whereas under normal conditions they do not exist at all. It is possible to practise this art of

attention until overtones can be readily heard, but in this there is nothing remarkable. Nor is support given to Bühler's hypothesis by the fact that psychologists who, under experimental conditions, have practised seeing transitional experiences—such as those involving the comparison of distances—can have transitional experiences in their everyday lives. Helmholtz, indeed, found that for a time his enjoyment of polyphonic music was much disturbed by the insistence of the overtones which he had learned to analyse.

Thus a reference to clang-analysis does not overcome the difficulties of the hypothesis. Yet all these difficulties disappear when we reflect, as proposed above (cf. p. 236), that under special conditions transitional experiences arise within the configurative phenomena themselves. Under these special conditions we do have a close analogy to the hearing of overtones; although now the analogy agrees rather than disagrees with the position we have taken. In other words, we can find no ground for assuming that transitional experiences exist where they are not observed; and even when they are observed, the original experience—the configural phenomenon — instead of disappearing, remains unaltered, just as does the clang-colour of a sound when we listen to its overtones.

I hope I have been able to convince the reader that there is no need of calling upon transitional experiences to explain phenomena in which these experiences are not observed; and furthermore that a far simpler and much more evident explanation can be given in terms of configural function. But the facts of the case are even more favourable to a theory of configuration than the foregoing argument might indicate; for with the progress of psychological investigation more and more instances have come to light in which the *effects* of configurations have been discovered where there was no possibility of referring them to transitional experiences. I need give but one example, similar to the

previously cited case of two grayish colours of different brightness. Suppose we try to find out how much colour must be added to a certain neutral gray of a definite brightness in order that it shall become just noticeably coloured. The minimal noticeable increment of colour is then called the colour-threshold. What we find is that the configuration of the whole phenomenal appearance exercises a marked influence upon this threshold; (for the colour-threshold is dependent, not only on the brightness of the neutral gray with which the colour is mixed, but also on the brightness of the uniformly gray background upon which the neutral gray that is mixed with colour has been placed.) We find, indeed, that the threshold is at its minimum, and the least amount of colour-admixture required, when both the neutral gray and its background are of the same brightness. After adding to a medium gray upon a background of the same brightness the minimal amount of colour necessary to make it barely noticeable, the colour will immediately disappear as soon as the background is replaced by another which is either brighter or darker. This result can be stated as a law of configuration by saying that (the greater the configurative difference between the brightness of a field and its background, the higher is its colour-threshold, and the more difficult it is to produce a colour-configuration.[239] Thus we see that colour-configurations are effective even where transitional experiences are altogether lacking.)

The reader must pardon the detail in which this problem of the transitional experiences has been set forth, because its importance for systematic psychology is far-reaching. Let us now apply the hypothesis of the transitional experience to the experiments with animals.

Two grays, A brighter than B, are placed before the animal. When the animal glances from A to B it will experience a darkening, and when it glances from B to

A, a brightening. Since B is the colour which is to be allowed, training will consist in establishing a connection between a certain kind of behaviour and the experience of darkening. The transitional experiences of brightening and darkening ought, however, to occur in accordance with whichever direction is taken by the wandering gaze. The question therefore arises why one of these transitions should be preferred to the other. The answer has appeared so obvious to writers on this subject that they have not taken it up at all; and, indeed, the training-theory has but one explanation. When the fowl has the transitional experience of brightening, both the eye and the head have been moved from B towards A. The head is therefore directed upon A, and the fowl should begin, wrongly, to peck at A. On the other hand, with the experience of darkening the fowl turns from A towards B, and this time pecks at B, and secures food. That is to say, the objective condition that the appropriate transitional experience finds the fowl's head nearer to the right gray than it is to the wrong gray is alone responsible for the fact that the fowl can be trained by this means. Again we are faced with a mechanistic interpretation, the validity of which is so doubtful that I do not believe it can be made to agree with what is actually observable in these experiments with hens.[240] In the first place, we have to assume that the hen works like an automaton. Consider, now, the case of Sultan with his double-stick. How could "training" have induced his behaviour? At the first attempt, without a single trial, Sultan took the larger tube in his left hand, and the smaller one in his right.[241] Accordingly, when Lindworsky writes that "the first achievement [that is, the differentiation of the larger from the smaller tubes], in view of a possible explanation in terms of transitional sensations, can not be regarded as indicating any insight," [242] we may add that this "possible explanation" not only must be rejected on its

own account, but, in this special case, it altogether fails.

Our discussion of the subject has secured our conception of the original nature of these configural functions against further attack. We are consequently now in a position to refer in passing to a problem which faced us in a previous chapter. If configural functions are primitive, we ought also to find them in those original modes of behaviour described as instinctive; and so, indeed, we do. As already noted (above p. 100), the stimuli which arouse instinctive action need not give rise to "simple sensations." If, for instance, a spider is put to flight by the approach of a bee—no matter what position the bee may occupy with regard to the spider's eye—an explanation of this behaviour must be found in a very simple configural function which recurs in each and every one of the innumerable positions of the bee. The problem then is to find out what may be the characteristic features of these primitive configurations.

§ 8—*Bühler's Stages of Development and the Principle of Configuration*

We have demonstrated that learning always involves some new achievement, and our discussion has dealt with a kind of learning which can be said to involve insight. But there are also many achievements at a much lower level of development which likewise demand a similar interpretation.

Here we find ourselves again in opposition to Bühler; for although he does not deny achievement with true insight, he advances a theory of stages of development. Below the upper stage of *intelligence*, 1
described as a capacity to make discoveries, he introduces a stage of *training*, which involves mere associative 2
memory, and below this still a lower stage of *instinct*. 3
Bühler believes that of instinct and training each has

its advantages and disadvantages. The advantage of instinct is the certainty and completeness with which it works the very first time it is tried. The advantage of training is its adaptability to special conditions of life. Coupled with these, however, are the disadvantages of inflexibility in the case of instinct, and of "inertia" in the case of training—the "inertia" being shown in the fact that learning by habituation is a slow process. The advantages of both the lower stages are united, however, in learning at the highest stage, which he calls intelligent learning.[243]

Bühler's three stages afford a valuable insight into the course of mental development, and, after removing certain blemishes, we shall find his hypothesis altogether acceptable. First, let us ask what relation obtains between these three stages. It might be assumed that they represent three entirely distinct modes of behaviour, but this would signify that new functions are added to old ones in a manner difficult to comprehend. According to the more generally accepted views, the theory of associative learning and the theory of instinct are intimately connected. At present the consensus of opinion seems to be that instinctive and habitual behaviour take place by virtue of connections between definite pathways in the central organs of the nervous system. These connections are regarded as fixed in the case of instinct, and modifiable in the case of habit; a distinction sometimes thought of as evolutionary, on the supposition that the instincts are but the acquired habits of one's ancestors.[244] Moreover, even for Bühler, achievements of instinct and of training are fundamentally alike. Both depend upon certain bonds of connection; only in the first instance these are innate and fixed, whereas in the second they are acquired and variable. But in conceiving intelligence as a distinct function, Bühler stands more or less alone. Attempts have been made to reduce intelligence to the effects of association; and although we must deny this hypo-

thesis too, we do, of course, recognize the desirability of a single principle which would enable us to avoid the necessity of assuming three entirely heterogeneous modes of response. This single principle, which always plays the chief rôle, whether it be in the explanation of instinct, habit, or intelligence, is for us the principle of configuration. Consequently, the behaviour itself, with its inner "closure" and its definite direction, becomes the essential feature in every explanation we shall have to offer, just as it has already served us in explaining the relation between instinct and reflex. The principle of configuration, which has proved its validity in explaining acts of intelligence, is found applicable to the explanation of lower forms of behaviour. Although this completely reverses the usual mode of procedure adopted in explanation of the most primitive modes of behaviour, the principle of configuration must not take an anthropomorphic turn—as if a dog, for instance, were only a very stupid man, a notion as foolish as it would be to regard man as a very clever dog; for not until we have worked out the common features in the behaviour of both dog and man, can we describe and define the difference between them. The assumption which we make is that intelligence, habit, and instinct depend upon differently conditioned and differently acting configurative functions—not upon different kinds of apparatus which, as Bühler assumes, are set going when needed.[245] We shall now consider how these differences may be conceived, and how the distinctions drawn by Bühler can issue therefrom.

Let us begin with the "inertia" which Bühler attributes to habituation. How can we explain the fact that so-called mechanical learning requires so much more time then intelligent learning? Even Ruger's experiments (cf. pp. 193 f.), which were very like those of training, indicated a descent in the time-curve only when a performance achieved by chance was also under-

stood. This agrees with Köhler's remark upon the choice-training of his animals: "If we attributed the time or the number of trials, in short, the 'work' done by the chimpanzees and hens in learning this kind of task, to the establishment of associative bonds (between a certain configuration and a reaction), our estimate of the essential achievement of the animals would not be high enough, because *the chief task of the chimpanzee in 'choice-training' is the discovery of the relevant material of which he is to make use.*"[246] Before connections can be made between certain reactions and certain stimulating objects, these objects must become outstanding features of the animal's phenomenal world. The total environment of the animal contains not only the critical stimuli but many others. Taken all together, these stimuli do not call for *adequate* phenomenal organization unless the perceiver knows, as the experimenter does, what it is all about. Since the field of vision of an animal must be quite differently organized from that of the human investigator, there is no reason to suppose that the stimulus-situation alone would provide the connections requisite for a relevant reaction.

This conclusion is confirmed by the evidence of learning-curves, that is, by the distribution of right and wrong responses on the part of animals. Although at the beginning right and wrong choices follow one another in a purely chance order, at some point a change suddenly takes place, after which virtually no errors are made. In an experiment with Chica, for instance, among fifty choices before this change occurred, twenty-five were wrong, while after the change had taken place only four errors were made. Such behaviour, described for the similar experiments of Yerkes on apes, Thorndike on monkeys, and Helene Frank on infants and young children, corresponds throughout with the characteristic appearance of the true solution in the tests of intelligence. Köhler's inference is supported by his further observation that "the greater the number

of different choices Sultan had learned to make with a pair of objects bearing different marks, the quicker he could master a new problem the material of which was not too difficult; and the same can be said of the other animals." [247]

The reason why habituation requires so long a time is that the conditions of the external surroundings, or of the internal organization of the animal, exclude the possibility of immediately apprehending the relevant parts of the situation. On the one hand, repetition is necessary to improve the inadequate conditions of chance in organizing the phenomenal field. On the other hand, many of the formations required in training are so arbitrary or so nonsensical—as in the case of nonsense syllables—that their internal cohesion is necessarily weak, and can be fixed only in the course of repetition. After a certain configuration is aroused, repetition makes it firmer and more readily available. But the statement that repetition "serves to strengthen certain connections" is false; because, prior to the appearance of the configuration, repetition is repetition of external conditions only, whereas after it has appeared, repetition is also a renewal of the configuration.

This conclusion seems to agree better than any other with the known facts. We know, for instance, that in a purely habitual achievement, like that of mechanically learning a series of nonsense-syllables, a "collective apprehension" [248] is requisite, in which the several members are bound together in a unified whole. Usually this construction of a unity occurs in the form of rhythmical groups, but in general what we mean to say is that in order to be learned the material must first receive some kind of organization,[249] every facilitation in the construction of which is a facilitation of learning. Likewise the "moments of grasping," which Aall finds so essential to memory, can be understood without difficulty as the learner's application of certain familiar principles of configuration to his material.[250] Further-

more, the following interesting result was secured by A. Kühn in an investigation made in the psychological laboratory at the University of Berlin. It is a known fact that in learning a visually presented series of words or nonsense-syllables the learner never confines himself merely to reading the material over, but soon begins involuntarily to recite it silently. In this way he both anticipates what is to come, and reaches back for what has gone before. Whenever the observer is forbidden to employ recitation, he finds himself unable to learn the series, no matter how often the material is read over. Indeed, the accumulation of mere readings in these particular experiments seemed to be harmful to retention; for the oftener the material was "merely read," the more repetitions by the "recitative-method" were thereafter required before it could be learned. The effectiveness of recitation rests therefore upon the fact that "*it leads to a more fundamental and more many-sided working-over of the material.*" [251] Finally, K. Lewin, as a result of ingenious experiments carried out in the same laboratory, reached the conclusion that "the learning-process can not be conceived as a connection between separate items. . . . Instead of learning 'syllables,' one learns 'to react to a given stimulus with a definite response.' . . . *The way is being practised which must be followed later in the reproduction.*" [252]

The "working-over," and the "way"—these terms are equivalent to what we have called the configuration. With reference to the facts revealed in processes of mechanical learning, we are therefore led to conclude that *all learning requires the arousal of configural patterns.*

When we have thus set aside, as not basic, the presupposition of the principle of trial and error, which is the principle of "frequency," it follows that repetitions without the achievement of a configuration remain ineffective whenever they are not positively harmful. Practice means the formation of a figure, in

the broadest sense, rather than the strengthening of bonds of connection.

To our conception of Bühler's stages of development we can also give an adequate physiological foundation. Again and again in this chapter we have met with the difficulty involved in a physiological theory of association (cf. above pp. 173 f.). We have seen how the behaviourist, in order to master this difficulty, has been led to reject the essential concept of association as a connection that is established by the individual in the course of his experience. Indeed, more than twenty years ago von Kries pointed out that the arousal of associations can not be explained on the basis of a mere "pathway-hypothesis" which assumes that nervous excitations travel along fixed paths. We shall set aside the question whether an assumption of innumerable innate connections can be made to overcome the difficulties involved in the variable nature of the associations to be established; for, even so, there are still other objections which von Kries has brought forward against the pathway-hypothesis, and upon which Erich Becher has enlarged.[253] According to von Kries the pathway-hypothesis is inadequate, not only to the problem of establishing associations, but also to the problems of "associative effects" and "generalizations." With respect to the first of these von Kries has particularly in mind the problem of spatial and temporal forms. Two lines which meet are called an "angle," while each line taken by itself is only a line. The associative effect of the two lines is therefore not the sum of the associative effects of each line taken by itself, and this new product the pathway-hypothesis does not explain. Under "generalization" von Kries refers to a fact of learning which we have already discussed with reference to instinct—namely, the psychological similarity, both in appearance and in effect, of processes which physically are quite different. Having once seen a figure, we are still able to recognize

it after its position, magnitude, and colour have been so greatly altered that different pathways must now be involved, and the whole process must take place in a quite different way. As a matter of fact, no object is ever twice reflected in the eye in exactly the same way. Variations of this kind are so common that they apply to all learning. Von Kries's conclusion, which relates closely to our own, is " that in many ways learning can not be a matter of the development of pathways, which bring remote parts into connection, but must be something that can only be pictured as the formation of unified domains, facilitating the co-existence of various states." [254] In carrying out this principle hypothetically von Kries refers these phenomena to inter-cellular activities.

In his conception of achievement von Kries approaches very close to our position ; the main difference is his attribution of achievement in learning to separate cells, the processes of which can only be conceived as co-existent, although they are, of course, adapted to one another. We, on the other hand, find the essential feature to rest in the state of arousal, or in the course taken within the whole domain involved. Becher has pointed out the untenability of any hypothesis which would limit these functions to a single cell,[255] and concludes that no adequate physiological theory of memory is possible. A way out of the difficulty has, however, been indicated by Wertheimer's hypothesis of a *configurative* physiological process ; and in his book on physical configurations Köhler has shown that this hypothesis can readily be applied to our knowledge of physics. Furthermore, K. S. Lashley has performed a series of experiments, the results of which are in full harmony with the view we have taken. Lashley's experiments consisted in tests of learning and retention in rats and monkeys after certain parts of the brain had been extirpated. Without going into details, we shall simply quote Lashley's conclusion : " As the

habit is established there comes into being a *definite structural modification* having topographical position and capable of destruction by brain-injury. The learning process is independent of locus, whereas the mnemonic press or engram has a definite localization." (The italics are mine.) [256]

As a consequence of these new hypotheses and results, objections to a physiological theory of association no longer force upon us the acceptance of psycho-vitalism; [257] instead, they open to us a new way in which association may be explained in terms of the physical configurations of the nervous system. These configurations, having already served us in the explanation of instinctive activity, will now prove of special value in clarifying the achievements of intelligence. From all this it follows that instinct, habituation, and intelligence, instead of being three different principles, are the expression, in different forms, of one and the same principle.

The difference emphasized by Bühler between intelligence and habituation, namely, the "inertia" of habituation, can now readily be explained, and we shall have occasion to describe it at greater length in the next chapter. The other criterion, of habituation and intelligence taken together, in contrast with instinct—namely, the capacity of adaptation to external conditions—can easily be made to conform with our hypothesis.

As primary conditions of behaviour the nervous systems of animals vary enormously. The result is a tremendous variety of types of behaviour. Consider the receptor-processes of this system which, according to our previous discussion, play so significant a part in determining behaviour; here we meet with differences of both kind and degree in the articulation and differentiation of these processes. A system capable only of a low degree of articulation and differentiation must possess a low degree of variability. Under these conditions the receptor-processes are unable to follow

many of the changes which take place in the physical environment. Strictly speaking, the intelligence of an animal depends upon the configurative character of its receptor-processes. In order that the needs of the moment may be intelligently satisfied the phenomenal field of an animal must vary in conformity both with its needs and its physical field. Yet expedient though it may be to prescribe certain criteria of intelligence for a special purpose, it is quite impossible to determine when intelligence begins. We can not say that intelligence begins where instinct leaves off; for this would over-emphasize the inflexibility of instinct. Studies of the instinctive behaviour of insects show that there is little or nothing of clock-work precision about it,[258] and our criterion of intelligence has its application here just as it does to the behaviour of man.

If we compare the mechanistic view of mental development with the three-stage theory of Bühler, we may call the former unitarian and the latter pluralistic. How then shall we characterize our own theory? It is pluralistic in so far as it embraces an indefinite number of different structures, and many forms of configurative change; but it is not pluralistic in the sense that it assumes a number of distinct faculties, like reflexes, instincts, habits, and intelligent acts. It is unitarian, not in the sense of reducing each process to the mechanism of certain neural bonds or associations, but in the sense of attempting to give an ultimate explanation of development in terms of the universal law of *Gestalt*.[259]

Chapter V

SPECIAL FEATURES OF MENTAL GROWTH

B. The Problem of Memory: Learning in Children

§ 1—*The Function of Memory, and its First Appearance*

Of the two main problems of learning, we have already endeavoured to clarify one; namely, the problem of achievement. Before we proceed to consider in detail the acquisitions of the child, we shall take up the second of these essential problems, which is that of memory.

When we speak of memory in everyday life we (a) generally refer to the remembrance of past events by a process of "imagination." For instance, I can remember a friend who has died. I can see him, as it were, in my "mind's eye," and can hear him speak again in the old familiar tone of voice. The characteristic of this remembrance is a phenomenon to which there attaches an *index of the past.* The experience I have imagined indicates the time at which it actually happened—as, for instance, long ago in the days of my youth—and also in the same spatial location—in the forest at N., or on the lake at Z., or somewhere in Berlin, or in the Alps, or elsewhere. Descriptions such 1) as these indicate that references to time and place can greatly vary in definiteness. The reference may be relatively definite, as, for example, on the day of my examination at the door of the examination-room; or 2) it may only approximate the time, as during my student years in Würzburg. But at least it is always

to something previously experienced. We speak ordinarily of remembrance, however, even when these indications of time and place are lacking. One may be able to remember Kepler's laws, or one may be able to remember the solution of a particular problem. Remembering in the latter case means that one can solve the problem without necessarily remembering when and where one learned its laws. In short, we speak of remembrance when, by reason of a previous experience, we are able to solve the problem or name the laws in question without first looking them up in a book.[260] (3)

But remembrance is not the only way in which memory can lead us beyond the present; for not only the past but also the future is experienced. I see lightning and await thunder; I hear the signal-bell in the theatre, and await the rise of the curtain. Expectancy may be considered a further accomplishment of memory. But that expectancy need not always depend upon memory, has already been shown in Chapter III, with reference to the analysis of instinctive reactions (cf. pp. 104 f.); and the conclusion there reached holds true for intelligent reactions. The configurations to which reference was then made included temporally extended patterns. When an animal drags a box to a point below a stick suspended in the air, this act already implies progress towards the goal, in the attainment of which a stick is needed, although the animal has had no previous experience of this kind. In the first solution of such a problem each part-reaction is made as a member of—or with reference to—the solution as a whole. Perceptual experience furnishes numerous examples of this; so, for instance, in hearing an entirely new melody we soon find ourselves expecting how it will proceed. 4) 6) R

But remembrance and expectancy in the forms explained do not yet exhaust the faculty of memory. Thus far we have considered memory as more or less independent of perception—as the occasion for

"memory-images," or whatever one chooses to call these non-perceptual phenomena. But still another and not less important function of memory is revealed (C) in perception itself. I go down the street, and meet many strangers. Yet here is a familiar face; there is my friend X; and over there is the lady who sat next to me yesterday in the street-car. Thus memory lends a character of familiarity to perceived objects, which again may greatly vary in definiteness from a mere quality of familiarity, as in the first instance above, to the complete assurance of the second; or again to a familiarity possessing either the characteristic of remembrance, indicated by the lady of the street-car, or of expectancy.

This perceptual accomplishment is not necessarily restricted to an individual cognition—or "recognition," as it is called; for when I apprehend a rose as a rose, or a piece of chalk as chalk, my perceptual phenomena also owe a considerable part of their essential character to memory. In order to understand this fact we need but observe how in time an object such as a new piece of apparatus alters its appearance—one might almost say its physiognomy—as a result of daily handling. It is indubitable that memory penetrates throughout the entire range of our perceptions; and certainly this effect of memory, in contrast with the one previously referred to as a "memory-image," is tied to the perceived object.

But the achievements of memory are not yet at an end, for hitherto we have confined ourselves to the inner aspects of behaviour—that is, to phenomena of experience; whereas objective behaviour also is shot through with memory. I need only refer to an example employed in the last chapter. If I do not drown when I find myself in deep water, it is because I learned to swim in my youth. Here memory works altogether without the aid of any revived experience; for long before I reach a decision, my arms and legs

are already carrying out their appropriate responses. After I succeed in coming to the surface, and have again filled my lungs with air, it may occur to me that this or that particular stroke would be useful or skilful, and thereafter I can regulate my swimming accordingly. In this way the phenomenal effects of memory have their influence upon motor behaviour. But swimming has taken place before I could recall anything of my past experience.

The achievements of memory are accordingly threefold:

i. The participation of consciousness, which may be more or less definite.

ii. The relation of this consciousness to perception—that is, whether the memory is free or tied.

iii. Certain kinds and degrees of positional and temporal definition.

With these distinctions in mind, let us consider the development of memory in the life of the individual. After birth the infant's behaviour shows that (i) conscious memory participates but little; (ii) when it does it is tied to perception; and (iii) is without temporal or spatial definition. First of all, the infant perfects its motor performances which soon involve a true component of learning, however slight may be the necessary degree of consciousness. On the phenomenal side, memory shows itself first as a quality of familiarity; 1 and perhaps even earlier, as a quality of strangeness. 2 If one brings an infant into a strange room before the completion of the first half-year, its behaviour noticeably alters. The infant looks around the room with a wide-eyed "astonishment," which disappears as soon as he is returned to his usual surroundings. The effect of the memory of his usual surroundings is indicated here by an impression of strangeness; but the basis for this impression must already have existed, because

his memory would have been the same had he never been taken into a strange room. How is this effect of memory to be understood? Perhaps the best answer has already been found in the distinction drawn between the "background" and the "quality" of primitive phenomena. The effect of memory derived from accustomed surroundings will then be this: the "background" of consciousness acquires the peculiar character of a relatively fixed level upon which separate phenomena make their appearance. "Astonishment" follows whenever this level is altered. The conception of the *level* has a wide application in psychology; a change of environment which involves the level is radically different from one which involves the qualities that emerge from it.

Within the first six months of a child's life one can also observe signs of smiling when the infant sees its mother or some other familiar person; and, on the other hand, signs of avoidance and displeasure when the infant is brought in contact with strangers. Here the participation of consciousness is apparently greater; because, on the one hand, the response is no longer determined by the background alone, and, on the other, a negative reaction to strangers is opposed to a positive reaction to persons with whom the child is familiar.

The next step, I should say, is one that adds to the character of familiarity a temporal definition which it did not previously possess—this being an expectation directed towards the *future*. It would perhaps be preferable to speak of a forward direction; because, as Köhler has shown,[261] there is a difference between a "future" that lies beyond a present definite and coherent course of events, and a "future" that is merely the continuation and end of such a course. Köhler employs the term "future" only in the first sense, whereas the achievement of which we are now speaking is of the second kind. Stern insists[262] that

references to the future take place earlier than references to the past; but I think he infers the independence of memory from perception too easily when he calls these first expectations "ideas." Let us take an example. Stern's daughter Hilda, as early as the age of five months, put out her lips when the spoon she was fed with was offered to her; although at first it had been very difficult to accustom the child to eat with a spoon. Instead of speaking of images of expectancy, we should, I think, describe this behaviour as follows. The child had learned to comprehend the process of feeding as a configuration in which the spoon played a definite part as a "transitional phenomenon." That is to say, the spoon as a phenomenon contained within itself a characteristic which carried beyond itself, just as a dark cloud appears not only black, but "threatening," without our having actually to imagine the oncoming thunderstorm.

Expectation, as a consciousness that something is missing, has as little need of "free" imagery as have the corresponding phenomena of familiarity and strangeness. Miss Shinn reports of her niece at three months that "she was much interested in a guest, a lively girl, and not only followed her movements, but would look for her when out of sight (89th day)." Stern concludes from this that the impression must have lingered afterward in the fainter form of an image, but I doubt it. In view of the very early age at which this observation was made, the existence of images, or phenomena independent of perception, seems to me very improbable. I think a more adequate description would be that a very vivid situation suddenly disappears, and in its place there comes another which has as its chief phenomenal characteristic, a "blank," or a "lack of completeness." This description appeals to me because it agrees with Köhler's observations that "one of the most frequent undertakings of a chimpanzee is to search for something not present." I can say the

same of my dog, which again and again will interrupt its play with people in order to seek and retrieve a plaything, such as a ball, a stone, a bone, or a piece of dog-biscuit. In all these instances we have a continuation of a "present course of events." [263]

It is not at all certain when the first "free" images are employed. Remembrance unquestionably occurs at the beginning of the second year, and with it the first definite relationship to the past. But whether remembrance is at first connected with perception, as to me seems probable, can not be decided from the factual data at hand.[264] It is equally difficult to decide whether the first "free" images are, or are not, images of expectancy. Certainly the first remembrances become definite very slowly; so that even for a four-year-old child a remembrance of yesterday is difficult, and one of the day before yesterday, impossible. At this age there exists a vague impression of incidents long past, likewise a rough distinction between before and after, and occasionally one between to-day and not to-day. The characteristic of place is better developed than that of time—"That was in Berlin," "That was in London," etc. All remembrances are, indeed, members of larger complexes, and carry their "membership-character" along with them.

Images without temporal and spatial relations, such as we adults use to aid us in our thinking, might be expected to occur very late in the course of development. I prefer not to consider the so-called "images of fantasy" in this connection. When a child understands a story and re-tells it (and the story-age begins with the fourth year) [265] the images that occur can scarcely be called non-temporal, because their employment is hardly different from that of images referring to the child's own distant past. These images of fantasy, however, mark progress to the extent that instead of going back to individual experiences, they

are first called into being by a story; otherwise they seem very like memory-images.

The memory of children also develops so as gradually to span ever-increasing periods of time. This subject has been thoroughly investigated by Clara and William Stern, who find that progress is shown in recognition as well as in what may more precisely be called remembrance. Recognition has precedence, and thus is disclosed the fact that it is a more primitive type of behaviour than remembrance.[266] Furthermore, it has been shown that the motives for remembrance undergo development; at first, remembrance attaches to perception, and only later to "images." In the beginning the child is passive with respect to his remembrances, but gradually he learns to control them, so that voluntarily, or upon being questioned, he can recall to mind definite events.[267]

Reference should be made, finally, to a peculiarity of youthful memory which Jaensch and his students have investigated in an extensive series of valuable studies.[268] Youth often shows a capacity for a visual, and also, though less often, for an auditory type of image, which is unique in quality, inasmuch as a perception can be voluntarily reproduced with full sensory clearness after a shorter or a longer period of time. Among 205 boys, ranging from ten to fifteen years of age, this capacity was indicated in 76, or 37 per cent. At what age this "eidetic" disposition, as Jaensch calls it, appears we do not yet know, but the investigations thus far made lead us to think that appropriate experiments can be successfully undertaken with very young children. Among the many different results attained by Jaensch, we shall mention but a few. Even "sense-memory does not retain the material presented without a selection having taken place. The achievement in this respect is not dependent, for instance, merely upon the frequency of presentation and the insistency of the object, but rather upon a selection which is directed from a

definite point of view." One of these "points of view" is that of *objectivity*, which in many individuals is so strong "that in the investigation of colours we must give up the usual apparatus of scientific optics, and rely rather upon such things as *flowers* for our stimuli; because these, rather than homogeneous papers of the same colour, give rise to the most definite images."[269] Furthermore, it appears in these experiments that the perceptions of "eidetic" individuals may be influenced in a manner interpreted by Jaensch and his followers as a fusion of the perception with the eidetic image. Indeed, Jaensch goes so far as to call the "eidetic" image the original, undifferentiated, unit from which perceptual, ideational, and physiological after-images are derived. I can not enter into a theoretical discussion of this point; but to me the main result of the work at Marburg is this: The perceptual configurations of youth are so firm that an alteration of any part of them will profoundly affect all the other parts, even though the result should be in direct opposition to the conditions of stimulation.[270]

§ 2—*The Laws of Memory*

Since we have denied that association operates as an external bond between independent parts, we can no longer accept the law of association as it is usually stated—namely, that if the phenomena A, B, C, . . . enter consciousness several times together or in immediate succession, and one of them thereafter appears alone, it brings with it a tendency to reproduce the others; special laws being derived which regulate the strength of the tendencies which lead from one factor to another in the association. We now find it necessary to restate this law so that it may read somewhat as follows: If the phenomena A, B, and C, . . . have been present once or oftener as members of a configuration, and if one of these reappears while still possessed of its

"membership-character," it will have a tendency to supplement itself more or less definitely and completely with the remainder of the total configuration. What is meant by the limitation of "membership-character" relative to reappearance can be made clear by the following example. Suppose one were asked to name a tree which begins with "will—," and should answer "willow." This would be quite easy. However, if the membership-character of this syllable "will—" as the beginning of a word were lacking, and we were reminded instead of a single monosyllabic word, it might be difficult to proceed from "will," to "willow."

But reproduction can also take place in a quite different manner. In our example "willow" may result, not only because "will—" as an initial syllable supplements itself to form a complete word, but also because an attempt is being made to construct a word out of "will" in accordance with some appropriate form of the language. Here again reproduction occurs in such a way that the total configuration is produced from the initial member. Thus it is unnecessary that the completed form should have been previously experienced. It is precisely in this way that many "false" constructions appear in a child's speech, which do not belong to the language at all, and which the child has never before heard. These "words" are freely formed by the child in accordance with certain principles of construction with which he is familiar. From the mass of material upon this subject which the Sterns have collected, we may select the following examples: Hilda Stern, 3.8—*vergurtelt* = to fasten with the aid of a girdle; the same child, 3.9—*metern* = to measure with the aid of a centimetre stick; Günther Stern, 3.10—*maschiner* = a locomotive engineer; the same child, 4.4—*dieben* = to steal; S.S., 2.6—*es glockt* = the bells ring.[271]

This kind of reproduction, which has even less in common with the older form of association than the

first that we mentioned, is very important in the development and progress of thinking. It may also be remarked that Otto Selz has stated the law of reproduction in a manner similar to the one we have adopted. In his experiments, as well as on the basis of other well-known facts, Selz has shown that an explanation in terms of the "constellation" of numerous independent associations is entirely inadequate to the facts, although, as the recent publication of his second volume indicates, his laws are related to our subject more in a formal sense than in content. As Benary has shown,[272] Selz's theory is still fundamentally mechanistic.

The older doctrine of association embraced not only the law of association, but also reproduction by *similarity*. To be sure, one often spoke of association by similarity, along with association by contiguity, but association then referred to the process of recall rather than to the bond established between ideas. Since the term *reproduction* has been introduced, one need no longer speak of association by similarity; for the principle maintains that an idea A can reproduce an idea A^1, without previous association, provided that A^1 and A are sufficiently similar. The principle of reproduction by similarity, however, does not rightfully belong to the theory of association, because similarity is not an external, but an intrinsic connection; consequently a law which replaces all internal connections by bonds which are merely external would here be violated. For this reason, there has been no dearth of attempts to exclude reproduction by similarity altogether from the explanation, and to reduce everything to association by contiguity. The facts, however, do not warrant this procedure, and L. Schlüter,[273] working at Göttingen under the direction of G. E. Müller—one of the chief representatives of the psychology of association—has found new proof of the existence of effects which must be attributed to similarity. In

addition, the work of Rosa Heine [274] in the same laboratory shows that recognition can not be explained in terms of mere "bonds" of association. Indeed, it has for a long time been thought that some connection must exist between recognition and reproduction by similarity, and I myself have considered both achievements as special cases under a more general law.[275]

It is difficult to explain these facts by the theory of association, and especially by reference to its physiological frame-work. We have already seen that other results attributable to similarity have been made the chief objection to the theory of association by von Kries. On the other hand, a theory based on "configurations" encounters less difficulty because "similar configurations" are also found in physics; and the law of similarity need only mean that configurations once present will furnish conditions favourable to the appearance of others like them.

We are therefore led to characterize the chief facts of memory as follows: When a new configuration arises under fixed objective conditions, this behaviour of the organism is somehow preserved. Upon repeating the objective conditions, the configuration will accordingly arise much more easily and swiftly than it did the first time. It will also return when the external conditions change, and are no longer so favourable as they were at first, even though the conditions are so incomplete that they would of themselves give rise only to a part of the whole configuration.

§ 3—*Motor Learning: the Parts Played by Maturation and Learning in Walking*

Having cleared up some of the preliminary, theoretical, questions, we may now consider the development of the child himself, taking our examples from the four directions differentiated at the beginning of the preceding chapter (cf. pp. 160 f.).

We need not repeat what we have said in general

first that we mentioned, is very important in the development and progress of thinking. It may also be remarked that Otto Selz has stated the law of reproduction in a manner similar to the one we have adopted. In his experiments, as well as on the basis of other well-known facts, Selz has shown that an explanation in terms of the "constellation" of numerous independent associations is entirely inadequate to the facts, although, as the recent publication of his second volume indicates, his laws are related to our subject more in a formal sense than in content. As Benary has shown,[272] Selz's theory is still fundamentally mechanistic.

The older doctrine of association embraced not only the law of association, but also reproduction by *similarity*. To be sure, one often spoke of association by similarity, along with association by contiguity, but association then referred to the process of recall rather than to the bond established between ideas. Since the term *reproduction* has been introduced, one need no longer speak of association by similarity; for the principle maintains that an idea A can reproduce an idea A^1, without previous association, provided that A^1 and A are sufficiently similar. The principle of reproduction by similarity, however, does not rightfully belong to the theory of association, because similarity is not an external, but an intrinsic connection; consequently a law which replaces all internal connections by bonds which are merely external would here be violated. For this reason, there has been no dearth of attempts to exclude reproduction by similarity altogether from the explanation, and to reduce everything to association by contiguity. The facts, however, do not warrant this procedure, and L. Schlüter,[273] working at Göttingen under the direction of G. E. Müller—one of the chief representatives of the psychology of association—has found new proof of the existence of effects which must be attributed to similarity. In

addition, the work of Rosa Heine[274] in the same laboratory shows that recognition can not be explained in terms of mere "bonds" of association. Indeed, it has for a long time been thought that some connection must exist between recognition and reproduction by similarity, and I myself have considered both achievements as special cases under a more general law.[275]

It is difficult to explain these facts by the theory of association, and especially by reference to its physiological frame-work. We have already seen that other results attributable to similarity have been made the chief objection to the theory of association by von Kries. On the other hand, a theory based on "configurations" encounters less difficulty because "similar configurations" are also found in physics; and the law of similarity need only mean that configurations once present will furnish conditions favourable to the appearance of others like them.

We are therefore led to characterize the chief facts of memory as follows: When a new configuration arises under fixed objective conditions, this behaviour of the organism is somehow preserved. Upon repeating the objective conditions, the configuration will accordingly arise much more easily and swiftly than it did the first time. It will also return when the external conditions change, and are no longer so favourable as they were at first, even though the conditions are so incomplete that they would of themselves give rise only to a part of the whole configuration.

§ 3—*Motor Learning: the Parts Played by Maturation and Learning in Walking*

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about the learning of movements (p. 163), but will begin with the concrete instance of learning to walk. The first attempt at walking, and the first success in walking alone, are subject to great variation in the time of their appearance. The eighth month is usually early, and the fourth half-year very late, for beginning to walk. We say that a child *learns* to walk and, of course, he does learn many things in the course of his varied attempts at walking. But does he actually learn *walking?* If a child, ready to make his first attempt to walk, were hindered for a few weeks, as James has proposed, would he at the end of this time, when at last permitted to make the attempt, behave as clumsily as he does when he is not so hindered? It is highly improbable that he would; although the psychologically interested widower, upon whom James set his hopes for this experiment, has not yet been found. Superiority at the later date seems rather to depend solely upon maturation, and the clumsiness of the child's first attempts must therefore in part be due to the fact that the centres from which the movements of walking are controlled have not yet fully developed. Awkwardness also results from the as yet insufficient development of bones and muscles. Walking therefore seems to be an inherited type of behaviour; a conclusion which agrees with the fact that birds, too, are able to fly safely and fairly well when first they leave the nest. To be sure, the act becomes more complete with practice, and we should hardly maintain that if a child were, without injury to his muscles, prevented up to his sixth year from walking, he could then upon his first attempt run as well as his companions of the same age. But this does not lead us to assume that walking is altogether a matter of learning; for maturation itself requires a stimulation that can be had only through the activity of the parts which are maturing.

The facts in the case are indicated by an investigation of Shepard and Breed on the development of pecking

among chickens. If one understands by *pecking* the entire process of food-taking—the striking, seizing, and swallowing of food—a marked development in this complicated activity seems to go on during the first days after hatching. Beginning with the second day small grain was presented to the chicks, and from day to day it was carefully observed how many attempts at pecking were successful. Among fifty such experiments in one group of chicks, the average of successful attempts was as follows: In the trials on the first day, 10.3; in those of the second, 28.3; in those of the third, 30; in those of the sixth, 38.3; and in those of the fifteenth, 43.2. For comparison, other chicks were tested which had been artificially fed for several days, and then allowed to peck their food for the first time. The result was that, although their performances at the start were no better than the first performances of the control-group, an improvement followed much more rapidly. One chick, which began its pecking four days after the normal group, exceeded their performance on the next day. From these results it would seem that maturation without stimulation can accomplish little; yet I believe that a large part of the improvement must still be attributed to maturation, inasmuch as all the chicks were about equally efficient after the sixth day, although individually they varied greatly in the amount of their practice. In order to be effective, however, maturation requires stimulation through the functioning of the act itself.[276] The issue has been somewhat obscured by the recent investigation of Dorothy Moseley who, in repeating Shepard and Breed's experiments, reached results which she interprets as being directly opposed to those of her predecessors. Certainly the delayed animals proved to be in no way superior to the others; but at least some of their part-activities were acquired so quickly that my conclusions seem still to be tenable. However, further experiments are desired to clear up the point.

That in learning to walk there is still something actually to be learned, is clearly indicated by an observation of Binet, who found that the older and weaker of two sisters—a first child—walked alone at an earlier age than the younger child ; the reason being that the older one gave her whole attention to the matter ; carefully choosing her objects of direction, and then proceeding to march with the greatest seriousness from one object to another. The younger child, on the other hand, was very vivacious, and would strike out without considering or attending to what she was doing.[277] This observation upon the influence of attention on learning to walk indicates that something was actually being learned, although we do not know precisely what. We may assume, however, that it was less the movements of walking themselves, than the inclination towards the goal, and the adaptation of means to this end. Walking is also a sensori-motor achievement in so far as it is locomotion towards an object desired or away from one to be avoided. In this respect too, walking need not be learned. Locomotion towards a desired goal is one of the earliest reactions, even when it is only virtually, not actually, possible, as Guillaume's detailed observations have shown. Thus, he reports : "The legs participate already on the eleventh day by extension in projecting the body forwards towards the seen-object. When held under the arms, the child makes complete walking movements from the thirty-fourth day onwards (lifting of the feet, extension and flexion of the toes synchronously with the foot-movements, etc.)." [278] This propensity to locomotion is not even restricted to walking, for it was recorded, by the same observer, of an attempt at clambering which occurred at the beginning of the fourth month.

§ 4—*Continuation: Grasping and Touching; Motor Configurations*

Movement-complexes of grasping and touching, which we shall now consider, are learned even earlier than walking. Exact observations upon the development of this behaviour have been made by Preyer, by Miss Shinn, and more recently by Watson.[279] The development takes a very complicated course, and passes through numerous stages. The original touch-organ of the suckling is not the hand, but the mouth. After the fourth week, everything that comes to the mouth is not only sucked, but worked over with the lips and tongue. This behaviour is no longer directly connected with the taking of nourishment; for if one puts one's cheek to the mouth of a suckling, the child if hungry will begin to suck; otherwise he will lick the cheek with his tongue. Pursing the lips (cf. p. 131), which is a feature of suckling, can be regarded as the most primitive form of grasping. Although we have already classified this behaviour among the expressive movements, there is no contradiction involved, but only an indication of the "indissolubility" of primitive behaviour which does not permit the sharp definitions that characterize the psychology of the adult.

Touching with the mouth assumes a growing importance, until all kinds of things are brought to the mouth by the hand. But this development does not take place all at once. A stage precedes at which the suckling brings only its hands to its mouth—according to Miss Shinn, this stage begins with the third month. It is interesting to note that at first this movement is not carried out by the hands alone; but as the hand is raised, the head is also lowered, so that the movement is clearly one of bringing hand and mouth together. The child is not carrying out certain definite hand- and arm-movements, but is merely uniting hand and mouth.

From Watson's experiments it would seem that an essential component of this early behaviour still persists after the child is able to bring other objects than his own hands to the mouth. Watson reports that on the 101st day a baby raised a stick of candy that had been placed in her hand, and poked it far back into her throat, which seems to indicate that the behaviour was completed only after the fingers had touched the mouth, rather than by the contact of the candy with lips and tongue.

The advent of grasping with the hand was observed by Miss Shinn during the twelfth week. If by chance an object came in contact with the hand it would be grasped and lifted, and then, after a time, let fall again. It was also observed that the mode of grasping depended upon the position of the hand with reference to the object touched. The eyes seemed to play no special part in this behaviour; for the child neither looked towards the object touched, nor towards her hands. At first the development of this behaviour seemed to be purely a matter of touch; though in the course of these movements, objects held in the hand frequently came by chance in the neighbourhood of the mouth. On the 86th day, Miss Shinn's niece tried for the first time to put a rattle into her mouth. On the next day this attempt was continued, the rattle being first raised to any place on the face, and then directed toward the mouth. After reaching the mouth, the rattle was then sucked in. It was noticeable, however, that the child could put her thumb in her mouth much more readily than she could the rattle. And yet almost three weeks earlier, upon her 48th day, a pencil which had been placed in her hand was carried six times to her mouth and energetically sucked by lips and tongue. From then until the 86th day, however, the child made not the slightest attempt to repeat this behaviour. Mention is made of this fact because *anticipations* of acts at a very early date, which only later are performed with

facility, are a common characteristic of the development of young children, and are of very great interest.

The achievements we have described are perfected slowly. The head also co-operates at the beginning; for instance, if the rattle chances to hit the nose, instead of the hand being lowered, the head is raised to bring it to the mouth. Yet the act always begins with a chance-contact of the object with the hand. If both hands happen to be touched, both are employed in raising the object, although this does not imply any true co-operation of the hands; for if the hands themselves chance to come in contact with each other, the one will be grasped and conducted by the other to the mouth.

After the 99th day the participation of vision in grasping was observed for the first time. Miss Shinn's niece then glanced down at the object while she was grasping it. Guillaume observed that a child of about this age would look at its hand when the hand was seized and held while on its way to the mouth.[280] In contrast, the direction of gaze toward a sound takes place at a much earlier date. As early as the 45th and 57th days, Miss Shinn reports her niece as looking around towards the keyboard of a piano that was played; but it was not until the 87th day that she glanced at the rattle which she already held in her hand, and whether the sense of contact was the occasion for this direction of her gaze remained uncertain. Apparently the gaze is directed much earlier by the ear than by the hand—a fact at least partly explained by our previous consideration of the matter (p. 85).

At a still later date, the eye begins to direct grasping; a long period ensues in which the eye confines itself to looking at the hands or at the object grasped, and the development of the behaviour of grasping something seen is very gradual. On the 113th day, Miss Shinn's niece looked at her mother's outstretched hand, and, with her gaze thus directed made awkward movements

with her own hand until the mother's hand was touched, grasped, and conducted to the child's mouth. How important the mouth is in this whole course of behaviour is shown by Preyer's relevant observation at this same stage of development—that the mouth would be opened before, or immediately after, the object was grasped—an observation since confirmed by Watson. Thus grasping after something seen is for a long time the first stage of an undertaking to bring a seen object to the mouth. This stage continues long with a characteristic awkwardness and lack of adaptability. The fingers, for instance, will be spread out in no position for grasping; the proper position being taken only after contact. During the hand-movements, the gaze is fixed upon the object. In a sense, there recurs in this part of the act the same type of behaviour that has already taken place in the simpler endeavour to introduce something into the mouth; though now the act is concerned with the adjustment of the object to the hand, rather than to the mouth.

Even after this behaviour has been practised, touch by the hand must still be substituted for touch by the mouth. At the age of seven months Miss Shinn's niece played with an object for the first time without carrying it to her mouth, but such behaviour was rare up to the end of her eighth month; and even far into the second year objects were occasionally brought to the mouth. Artificial means must be employed with many children even as late as the third year in order to wean them from this habit, especially if it be thumb-sucking. The direction of the hand by touch is attained very slowly—much more slowly than direction by grasping.

When we take this phase of development as a whole, it appears that a relatively complicated behaviour arises out of much simpler activities. And yet we are unable to agree with Preyer's statement that learning consists in nothing else than a partial isolation and re-

combination of already existing movements—in other words that learning is only a matter of *training* as this term is commonly understood. Bühler, for instance, regards learning to grasp as entirely dependent upon training. We now see why it has been necessary to prolong our theoretical argument concerning the nature of this form of learning; for, writes Bühler, "the entire acquisition of innumerable manipulations and activities which the child learns to master in early youth are executed in accordance with this principle of training, beginning with positional movements of creeping and walking, passing through the stage of grasping-movements, and culminating in the technical and artistic performances which constitute training in the narrower sense of the term." [281]

On the other hand, Bühler points also to the similarity between grasping and gazing; for he tells us that, just "as the eye-movements which bring the image into the clearest field of vision are released reflexly by means of peripheral light-stimuli, so the arm-movements which bring the objects to the mouth as the place of most sensitive touch, are released by means of the pressure sensations of the skin." [282] His statement refers to a stage of development in which seeing does not yet participate in grasping, and his explanation is given in terms of the formation of bonds between the pressure sensations of the hand and the kinæsthetic sensations of bending the arm. But we rejected this hypothesis in the case of eye-movements of fixation, and replaced it with another. Can we do the same with reference to the development of grasping and manipulation? The same problem arises when grasping is directed by the gaze. Here again, we are disinclined to accept an "empiristic" explanation, as Stern does. [283] Rather, we can assume a direct connection between the visual impressions, arm-movements, and tactual impressions, so that gazing will lead directly to grasping and lifting to the mouth. Guillaume also

rejects an empiristic explanation of this achievement, especially because of the rapid progress in learning, and also because in his first attempts the child always looks at the object, never at the grasping hand.[284]

Quite apart from the general considerations of the previous chapter, there are, in fact, a series of data which contradict explanations based upon bonds of connection. In the first place, the same objection can be raised against this hypothesis which has already been raised against the similar hypothesis in the case of visual fixation, that the number of connections would need to be enormous. The hypothesis assumes that an individual has learned to attain a certain end by movements which are explained in terms of connections, without first demonstrating that all the connections necessary for such an explanation actually exist. Von Kries attacks the hypothesis on these grounds.[285] With writing as an example, he points out that the innervation of the muscles required in writing even a portion of a letter involves a wide range of variability, according as we write large or small letters, quickly or slowly, energetically or easily, with this or that position of the arm, to the right or to the left, above or below on the paper. Von Kries also finds in this variability a decided objection to the "pathway-hypothesis." Furthermore, how can this theory of bonds explain the "anticipations" we have pointed out (p. 268)? An infant was observed to place a pencil in its mouth correctly, six times. Of course, this might be explained by bonds of connection between the several movements, if we assume that the act started each time with the same position of the arm. It might then be said that this sequence of acts occurred first of all by chance, and was retained during the brief period of time in which the performances were being repeated. But such a description is contradicted by Miss Shinn,[286] who reports that, after she placed the pencil in the resting hand of the child, "the hand closed on it at once (the thumb

description of behaviour

correctly reversed) and carried the pencil to the mouth. I had no idea that this could be more than an accident, but pushed the hand away from the neighbourhood of the face, lest the pencil-point should do harm in some aimless movement. To my surprise, the baby *six times* carried the pencil directly back as I pushed it away, and as she did so she put out her lips and tongue toward it eagerly, with sucking motions, much as when about to be put to the breast." This is the description of a good observer, and from it one must conclude that the same movement was not repeated each time in any such manner as would allow the same connections to function again and again. On the contrary, the behaviour was of a kind that would attain the same result each time it was repeated. Indeed, the process appears to be entirely imbedded in an instinctive mode of behaviour; for the child put out her lips and tongue toward the pencil with the same sucking movements that followed when she was about to nurse.

Reserving this observation of Miss Shinn's for later consideration, let us turn to another argument against the hypothesis based on bonds of connection. The hypothesis maintains that a movement originally carried out instinctively, or in any other way, enters as such into a subsequent performance which is being learned. Regarded as a movement, any unit must remain the same afterwards that it was before being incorporated into a subsequent process of learning. This presupposes that the course of behaviour is made up of separate and isolated parts, a presupposition which has its exact analogy in the sensory domain, where perceptions have been likewise conceived as a number of separable sensations. Our theory of configuration supplants this view, against which we shall bring still further proofs in this chapter. It may therefore be noted at once that an hypothesis which has failed to satisfy our needs in the sensory field, can hardly be

expected to agree with the facts adduced in the case of movement.

If a young baby imitates the movement of an adult by carrying out an action which he is otherwise able to perform spontaneously or instinctively, the imitative action is found to differ from the similarly constituted spontaneous action by a marked degree of awkwardness. Compayré notes this difference,[287] and the Sterns report of their daughter that, "if one says to the child when she is well disposed, 'erre, erre,' these syllables which she otherwise utters involuntarily and easily, will be repeated, but only after an apparent effort, which often lasts several seconds." [288] This difficulty can not be explained in terms of piecing part-movements together; for if that were what took place the action ought to occur quite independently both of the result and of the total situation.

In America, numerous experiments have been undertaken to study the learning of new acts; such as throwing a ball at a target; striking at a punching-bag; writing on a typewriter, or more simplified acts of the same order. Tests have also been made of writing under difficult conditions, as with the left hand, or so that one's own writing is only visible in a mirror. As noted in the previous chapter, the results of all these investigations indicate that the learning of a certain type of movement is not simply a motor affair, but that sensory components are absolutely essential to it. A further and generally confirmed result is the following: The more strictly motor a task is, the less has consciousness to do with learning it, and the more must the learner be directed upon the result, rather than upon the activity itself. Whenever one throws a ball at a target and gives attention to the throwing rather than to the target one is quite sure to miss the mark.[289]

In learning more complicated movements—as, for instance, writing ten words always in the same order on a typewriter—the course of learning is as follows.

At first each letter is sought and written for itself, that is to say, a mode of perception which we may call *seeking* becomes the centre of the whole action. This complicated process alters as superfluous movements are eliminated; but, above all else, as the act is learned, a complex unity supersedes an unconnected mass of particulars.[290] In this unification a "movement-melody" composes itself. The visual search for single letters disappears and attention is thereafter directed only upon the entire course of the procedure. Indeed, any special consideration of details always introduces difficulties. How far the visual aspect of learning may disappear is shown in an example given by Betz,[291] who had acquired a considerable practice in typewriting, and always used the same machine. Once, however, when he tried to see if he could write down from memory the picture of the key-board of his machine, his attempt was a failure. Not only had he the greatest difficulty in reaching any decision at all as to the appearance of the keys, but although in writing he never looked at the key-board, he made many gross errors in reproducing the order of the letters. In doing what we are accustomed to do, we are aware only of the errors we commit; then the wrong movements spring forth "as not belonging to the melody."

If we ask further how a movement-melody can develop out of a summation of movements, the answer is that it does so of itself whenever attention is rightly directed upon the goal, which is an object of the outer world. Thus the movements constantly vary in the direction of a better formation, which is achieved in a way similar to the achievements in efficiency described in Ruger's experiments. Although in Ruger's experiments improvement in learning was effective only when it was understood, here the case is different, at least in so far as the finer adaptations are concerned; for although these adaptations may occasionally be reflected

in consciousness, that has no influence upon the efficiency of learning, and to direct attention upon them only disturbs the performance. This does not mean, however, that attention has no influence at all upon this kind of learning. On the contrary, Ordahl has shown that attention of some sort is involved in the learning of quite simple movement-melodies.[292] But as regards the participation of volition and knowledge, Müller and Schumann found, as early as 1889, that neither is required in the composition of what they called a *motorische Einstellung* (" motor set "). If one lifts a lighter and a heavier weight many times in rhythmical succession, a " motor set " is gradually built up, inasmuch as the lifting of the pair of weights becomes a process in which the lighter weight is followed by a more energetic lift, so that the two liftings taken together have an iambic rhythmical character. The existence of this " motor set " was proved by tests made after the practice-experiments were over, in which the normal weight was paired with other weights within the same range of heaviness. On account of his " motor set," the observer found that if he lifted two equal weights, the second seemed much too light ; and only after it had been made considerably heavier than the first weight, did the two appear to be equal. The observer, of course, knew nothing of this " set," which nevertheless made the second impulse to lift so much more forcible than the first that the corresponding weight seemed much too light.

Two different methods of arousing " motor set " were tested by Ordahl. In one, the observer's attention was distracted from the practice of lifting the two weights —one of which was twice as heavy as the other—by reading to him an interesting story, the content of which he was afterwards required to relate accurately. As a complementary experiment the observer's attention was directed upon the weights. In the practice-tests a weight twice as heavy was employed as the second

member of the pair, together with two slightly heavier and two slightly lighter weights. The observer was then called upon to decide in each case whether the second weight was twice as heavy as the first, or more or less heavy. Under these conditions the " set " was, in fact, notably stronger than under the conditions of the distraction-experiment. We may also recall, in this connection, Binet's observation that attention contributes its part to a child's effort in learning to walk (see above, p. 266).

Taking these results together, our conclusion seems to be that in learning a more or less complicated movement a movement-melody must be composed ; that is to say, a formation after the manner of our " configuration " takes place, which does not consist of independent parts, but is an articulate whole. The " motor set " itself, which is explained by its discoverers in quite a different way—namely by association—proves nevertheless the correctness of our assumption. Consequently, a " motor set " arising under the conditions of a strictly rhythmical lifting of weights, regulated by the beats of a metronome, presupposes a configuration for the same reason that it has been found impossible to learn nonsense-syllables without constructing them into a configural complex. The relation of motor to sensory learning is also indicated by the fact that many of the laws of sensory learning have been found applicable to " motor sets," especially in the experiments of Laura Steffens—a pupil of Müller—and hence it can not be supposed that motor and sensory learning are derived from two quite different sources. Improvement in a performance ought therefore to consist in the construction of better and more complete configurations ; but the improvement is not attributable to intelligence. To know beforehand how we must perform the act avails us nothing ; for these configurations do not originate as " intelligent " configurations do. Their seat lies chiefly in other centres. And yet some con-

nection must exist between them and the centres of those processes which are accompanied by a high degree of consciousness. In beginning to learn, the phenomena of perception must be present, and the learner must have a fixed purpose. The configurative formation is then influenced by these components. Practice, or continual repetition, is requisite, and it is very apparent that repetition contributes essentially to the fixation of the behaviour. One has only to think how a musical virtuoso has to practice in order that his "fingers may not rust." It is equally apparent that repetition has still another object; it must create conditions favouring the arousal of the new configuration. In learning by repetition, the concept of chance—in the sense in which it is used in the theory of trial and error—will not suffice. Chance may help, but it is extremely doubtful if each new advance is really haphazard; especially when one considers how "wise" are the nervous centres which have nothing to do with consciousness, and how promptly and exactly they function in the face of sudden danger.

But a closer argument in support of this point of view would lead us too far. It is enough to note that new configurations are also attributable to the lower centres; as is demonstrated by the fact that the practice-curve improves by leaps. Such improvements occur most frequently when the animal is having a "good day." They embrace all kinds of learning, whether it be a motor performance, a mechanical accomplishment (Ruger), or an intelligent achievement (Köhler); indeed, the most difficult problems requiring intelligence are solved on "good days" only (see above p. 221).

Finally, according to Köhler, "there seems to be in apes a high positive correlation between intelligence and dexterity"; [293] a fact which would be very singular if a relationship did not exist between these two kinds of behaviour. Intelligence and dexterity are also both subject to great individual variations. The construction

of a "motor configuration" is specifically differentiated from an act of intelligence, chiefly in that a projection of the configuration prior to its performance is impossible. In this respect a motor pattern resembles the construction of a configuration in what is called "training," though it may far surpass the results of training in its refinement and precision. Acts of skill are often referred to as achievements of training, and there can be no objection to this description so long as one conceives training as we have done without mechanistic implications. In conclusion, I wish to emphasize the fact that very little is known about motor learning. Why a motor performance should improve itself is a question which at present can be answered only in very general terms.

Returning to the child's learning to grasp and to touch, these, too, are acquisitions of new configurations, and indeed all behaviour in which sensory and motor components work together is closely related to the experiments we have described. We can now explain *anticipations*. The configuration takes place when the objective conditions happen to be unusually favourable, and since these conditions do not repeat themselves, the configuration can only reappear when a change in the internal conditions has taken place; in which event the external conditions may be even less favourable than they were upon the first occasion. This statement will also account for the "anticipations" described above in connection with Köhler's experiments upon intelligence (see p. 213).

The history of this process reveals a close interaction of maturation and learning. On the one hand specific situations are necessary in order to produce new achievements; on the other hand the nervous system of the infant must also have reached a certain maturity before it can profit by these specific situations and react in an appropriate manner. The anticipations are good illustrations of this fact.

Two more points should be mentioned: (1) As Stern remarks, learning to grasp is different from learning to write. This difference indicates that the organism will develop on its own lines in such a way that when the specific situation occurs it will at once be capable of the more primitive grasping responses. Writing, however, presupposes that the organism is already capable of other achievements. Furthermore, much less specificity of situation is required for the first than for the second performance. (2) Maturation must not be taken as a process which runs its course quite independently of learning. According to our view, every new acquisition is a saltatory change in the organism. Since maturation goes on for a long time after the first acquisitions have been made, parts which have already become changed through learning are still being affected by growth.

In all these, and in similar processes of development, Stern notes a peculiarity of *conduct*; which is that the child is *voluntarily* engaged. Whatever "will" may be, the child's success depends upon his will-power. "Of two children, equally strong and healthy, and with instincts of the same stage of development, the one possessing the strongest will-power will more quickly perfect its behaviour." [294]

§ 5—*Sensory Learning: the Development of Colour-Vision*

With the aid of a few significant examples we shall now try to follow the course of development in the perception of the child, in order to see how a picture of the world as we know it gradually arises out of the primitive and diffuse configurations of early experience. It is a truism to us adults that our perceptual world is the sum-total of our experiences. The question is: *How* has experience brought this about? We must not forget, for instance, that the problem of experience

involves *achievements* as well as *memory*; and furthermore, that the possibility of effects attributable to *maturation* must always be kept in mind.

We shall begin with the investigation of colour-perception, for which great pains have been taken in securing a wealth of interesting results of great importance for a general theory of colour-vision. A large number of methods have been devised for these investigations; some depending entirely upon speech, while others are more or less free of linguistic aid, and can therefore be applied at an early age prior to the development of speech.

A. *Methods involving Language*

1. *The Word-Sign-Method:* Two colours are placed before the child and named for him. He is then asked to point to red, to yellow, etc. When the names of two colours have been learned, a third is added, and so on.

2. *The Naming-Method:* (*a*) As *directed* by the investigator, in which the child is shown separate colours to which he responds with their names; (*b*) As *spontaneous*, in which the child draws colours from a box, and names them himself.

3. *The Symbolic Method:* The child is told a story, a definite colour being shown him for each of the persons involved, with the remark that "this is the father," "that is the mother," etc. When the story has been related several times, the child repeats it, and at the same time points out the colours belonging to each character.

B. *Methods without Linguistic Aid*

1. *The Method of Arrangement:*

(*a*) By *Names*. A number of colours are placed before the child and he is told to pick out all the red ones (or blue ones, etc.).

(*b*) By *Samples*. A colour is placed in the child's hands, and he is told to select from a pile all colours

like the sample; or, the sample is mixed in with the other colours and the child is then told to find it again. The method of arrangement requires language only to the extent of explaining the problem to the child.

The last two methods, which follow, are copied from experiments with animals, and are applied entirely without speech.

2. *The Method of Preference:* Several different colours are placed before the child, and one observes in a large number of trials which ones he grasps, or toward which he glances.

3. *The Method of Training:* By means of rewards, one seeks to persuade the child to select a single colour, from several that are shown him. If the training is successful, the existence of a *sensory* achievement is thereby demonstrated.

In early infancy colour-impressions, although occasionally they give rise to strong feelings of pleasure,[295] play but a very slight rôle (the colour of anything being as yet unimportant as a means of recognition). Thus, Miss Shinn's niece did not alter her behaviour when, in her seventh month, a white pacifier was given to her instead of the customary black one. Reactions may, however, be called forth by colours. Very early the child turns toward bright objects, and begins to react differently to light and darkness. In this connection it should be noted that light and dark are not really colour-designations, such as black and white, but instead indicate differences in the "level" of the surroundings. All we can properly say is that a bright object may stand out readily from its "background" at a very early age. One also finds at this time that saturated colours are preferred to those which are achromatic (black-gray-white). Miss Shinn reports such a distinction at the end of the third month, while Valentine, investigating with the method of preference, in which the child was tested by the direction of his gaze,

confirms this finding in the fourth month. Valentine's experiments show, too, that colours are not all of equal value; for he was able to obtain the following series in order of preference: yellow, white, pink, red, brown, black, blue, green, violet.[296] This series indicates two things: (1) that the bright colours come before the dark ones—white before black, pink before red; and (2) that the long-wave "warm" colours are preferred to the short-wave "cold" colours. One might almost suppose that in the white-black series, not black but an intermediate like dark gray is least attractive, because otherwise it is hard to understand why blue, green, and violet all followed after black.

Holden and Bosse [297] employed the method of preference in an ingenious way by placing coloured squares on a gray ground of the same brightness as the colours, and observing whether or not the coloured squares were grasped. The result of their experiments was that the colours from red to yellow were grasped promptly by children seven to eight months old, but that the infants must be from ten to twelve months old before they will reach for the colours from green to violet. What can we infer from this result? It is clear that if a child grasps at a coloured square he must have seen something different from the gray background which was worth having; and the difference could not have been one of brightness, because the conditions of the experiment excluded this possibility. On the other hand, we can not infer that the child saw *red* or *yellow*, for we do not know that what was seen in the test with the red square was different from what was seen in the test with the yellow square. As regards the negative results obtained in the eighth month with the "cold" colours this much, at least, can be said; that the child did not perceive anything that was both different from the background and worth having. Since these same colours were grasped a few months later, it is at least improblable that this negative result involved seeing a

difference, without any desire to grasp the colour; for on such an assumption it would be difficult to understand why a desire for these colours should have developed so suddenly. The most probable explanation is that at first only the "warm" colours stand out against the achromatic shades; the "cold" colours being perceived later.

What colour-phenomena are experienced by the child at this stage of development? Putting the matter as simply as possible, the child experiences configurations of gray and not-gray; not-gray being like none of the colours we know and recognize, but something that differs from gray in the same way in which to us all variegated colours differ from neutral tones which have no colour at all. By the customary usage of speech *colour* means what we have designated as variegated—white, gray, and black being commonly referred to as "uncoloured." We conclude, then, that during the first three-quarters of the child's first year of life no configurations of colour arise other than a primitive chromatic-achromatic distinction; and, indeed, this configuration occurs only when objectively "warm" colours chance to lie upon a colourless background, or, we might add, upon an objectively "cold" ground of the same brightness.

Now, when a colour-configuration is also established for the short-wave colours, the question arises whether this phenomenon is like the one determined by the long-wave rays, or whether, in its distinction from this configuration, it also possesses the phenomenal characteristics of the "cold" rather than of the "warm" colours. This question can not as yet be answered with any degree of certainty, but, as we shall see, there soon occurs a stage in the child's life when a distinction between the "warm" and "cold" colours is made. I am inclined to believe, however, that at the beginning the "cold-figures" appear merely as undifferentiated "colour-figures." Several facts seem to support this

view. Learning the names of the colours is at first very difficult, and unless the child has been influenced by some special training, he does not know the names at the time when the distinction is first made. Names of colours may occasionally be employed, but quite promiscuously; whereas a colourless object is never given a colour-name. The Sterns report of their daughter that "at the age of three years and two months Hilda called bright and dark things *white* and *black;* otherwise she named correctly only the colour *red.* But the accuracy of the word *red* was obviously quite accidental, since *all variegated colours were likewise called red.*"[298] As Winch has noted, it often happens that variegated colours are distinguished from neutral tones by giving them all the same name, which indicates that all variegated colours have a common characteristic in contrast to the achromatic tones, and that this common factor must therefore be much more influential than any differences seen between the variegated colours themselves.[299]

With some reserve I may note the following observation upon myself. Being "colour-weak,"[300] I see red and green only under favourable conditions. There are certain colours which I recognize immediately as "coloured," yet they are always very distasteful to me, simply because I am unable to classify them. I am tempted to call them brown, though they easily slip over into red, or even into green. Yet they possess, as I have said, a quality which causes them to fall apart from all the other colours, although they remain unquestionably chromatic.[301]

Turning now to the experiments which involve the use of language, the numerous results of Preyer, Binet, Shinn, and Winch,[302] among others which are available, seem at first glance to be quite contradictory. It is impossible to give a complete explanation covering all these observations, because we are not sure of the exact nature of the colours employed by different investi-

gators. Future workers in this field should consider this point and take care in all examinations of colour-vision that differences which may be attributable to brightness and saturation are rigidly excluded.

An important material cause of the varying conclusions reached by different investigators can be found in the fact that the result of an experiment depends so largely upon the *method* employed. Word-sign-, naming-, and arrangement-methods give quite different results, as Binet and Miss Shinn have already pointed out. In each method, too, the number and selection of colours to be combined are of the utmost importance in determining the results of the test.

Binet's experiment will serve as an example. He began his investigations with a little girl two years and eight months old, placing before the child at first only red and green strands of wool (the Holmgren test). Examination by the first two language-methods produced 100 per cent. correct reactions. Yellow was then added, the result being that yellow and green were constantly confused. When the yellow was removed, all the reactions were correct, but as soon as it was included, the errors began again. If the green was now taken away the word-sign-method indicated no errors; but with the naming-method there were 100 per cent. mistakes, since yellow was always called green. On a day when the naming-method still indicated a complete confusion of yellow and green, the arrangement-method (Bl*b*)—in which a certain strand already shown was selected from a pile containing three strands each of red, yellow, and green—was carried out with no errors at all.

Up to the present these errors have almost always been interpreted as attaching wrong names to the colours. This explanation, however, appears to be insufficient; for why should naming have been so difficult? Apparently there are difficulties here which do not exist in learning other words. Furthermore, we

have already seen that the names *black*, *gray*, and *white* are never employed for variegated colours, provided the colours are sufficiently saturated.[303]

Other results show that frequent confusions of colour occur in the case of blue-green, green-white, yellow-white, violet-blue, red-blue (according to Miss Shinn), all pale colours with gray or white, and all dark colours with black. Finally, Winch performed a large number of experiments with the naming-method which previously had furnished only the most unfavourable results. Winch sought to eliminate the defects of the method by testing children who already had been taught the names of the colours in the kindergarten, where all the colours were practised equally. According to Winch, a difference in the serial order of the correct word-usage ought then to depend altogether upon a difference in the colour-phenomenon itself; provided, of course, that phonetic difficulties attaching to the particular names of the colours have been taken into account. The individual variations of his results were considerable; but on the average the following series was indicated: red, blue, green, yellow, violet, orange. Meumann found exactly the same order; while Garbini found the following series, both in naming and in discrimination: red, green, yellow, orange, blue, violet.

In considering results such as these, it is easy to assume, as most investigators have done, "that what we have here is the development of certain modes of apprehension, but not the development of a sensory capacity or, in physiological terms, the development of reactions of the 'visual substances' of Hering's theory."[304] The principal grounds upon which investigators have been led to this view are the following: First, the great variation in the results obtained from different observers. Secondly, the great individual differences. For instance, Miss Shinn's niece could name red, yellow, and blue things at the end of her 73rd week. Experiments were then begun in the 79th

week, and were at once successful in the case of these three colours. With Preyer's son, on the other hand, it was impossible for him to learn two colours at the end of the 87th week, and the experiments were successful for the first time in the 108th week. Thirdly, the dependency of the performance upon the nature of the test. Bühler also finds support for this view in an instance of anticipation observed by Mrs. Woolley. In its sixth month the child she observed indicated by grasping-tests a definitely graded preference for colours: "warm" being preferred to "cold," and dark to bright. This preference then disappeared altogether, and for many months no differentiation on the basis of colours was in any wise indicated. According to Bühler, "it would be without rhyme or reason to assume that the sensory capacity had undergone any retrogression." His argument, however, is based upon a presumption which we have already several times declined to accept; namely, the "constancy-hypothesis" that a certain sensation always corresponds to a certain stimulus just as soon as ever the capacity for the sensation in question has been attained. Only on the basis of this assumption is Bühler's inference valid; otherwise one might say that the conditions for the appearance of colour-phenomena in the case of Mrs. Woolley's child were especially favourable in the sixth month—a possibility which even Bühler makes note of. The grasping-tendency is at this time in the ascendant, and if a number of coloured papers are placed before the child it will glance frequently from one to the other before it grasps. But as development proceeds, the child is no longer restricted to grasping, but begins to undertake new types of manipulation with things; consequently the colours become entirely irrelevant. In other words, as soon as the conditions are no longer favourable for the appearance of colour-phenomena—or, better, of colour-configurations—the phenomena themselves fail to appear.

Whatever may be said against Bühler's special argument based upon anticipations such as Mrs. Woolley has recorded is also applicable to the general argument. We simply can not be satisfied with a description which states that the child experiences the colour-sensations correctly but is not yet able to apprehend or discriminate them. We ought rather to ask: What is the actual nature of the child's phenomena? Indeed, the case is the same as it was when we argued against "unnoticed relations" (cf. above pp. 228 f.). From our point of view the assertion that a colour-difference is apprehended signifies that two colours have entered into a definite kind of union; in other words, there arises a configuration of two colours, in which the colours appear as they stand in this configuration. The development of colour-perception is therefore the gradual construction of new colour-figures; accordingly the conditions for the arousal of such figures may easily become less favourable than they were at some previous time. The anticipations which have been described in so significant a way by Mrs. Woolley, and which have also been observed by other investigators, are in fact a demonstration of the validity of our theory; for we have already shown (p. 279) how anticipations can be understood as configurative processes arising under exceptionally favourable external conditions.

Regarding the results in this way, their dependency upon the method employed is also readily comprehended; as can be shown by reference to Binet's experiment which we described above. If the configuration red-green has been acquired, and yellow is then added, the confusion which takes place in naming may be looked upon as an indication that now the same configuration is operative; namely, that of red—not-red. In agreement with this interpretation, the word-sign-method indicated no errors for red and yellow, whereas by the naming-method all the results were wrong. When, on the other hand, the arrangement-method was

employed, the red configuration was no longer involved in testing the child with a yellow or a green. The "relational system" having been changed, everything now depended upon the configurations of yellow—yellow, or of green—green, or again of yellow—not-yellow, or of green—not-green. The arrangement-experiment teaches us, indeed, that variants of this kind occur without in any way contradicting the results obtained in the other experiments.

It is apparent that future investigators will have to take these configurations more into account than they have done in the past; both the kind of colours, and the ground upon which they are exhibited will have to be systematically varied.

A discovery by Köhler offers further support to our theory. In his investigation of chimpanzees he arranged some tests of choice-training in which he used colours A, B, C, not from the black-white series, but lying somewhere between red and blue, or between red and yellow. His results correspond exactly with those previously reported. One observation, however, is of particular interest. A, B, C, D, E—E being the reddest—were five different colours lying between red and blue, the nuances being easily distinguishable by man. Taking the pair B C, the chimpanzee was required to learn to react to the markedly reddish C. This attempt was a failure. The interval was then increased, and the investigation continued with the pair B D. The selection of D was then rapidly learned. When thereafter the pair B C was again offered, C was chosen correctly without an exception; and some time later, D was selected without an exception in the interval C D.[305] This result is very important to us for the following reason: At first it was impossible to construct a definite configuration of B C, although occasionally it proved effective; but the configuration of B D took place at once, and thereafter both B C and C D were effective. Here, then, is a case which corresponds ex-

actly to our law of memory as formulated on p. 263. A configuration arising under favourable objective conditions reappears also when the conditions are less favourable.

The following hypotheses concerning the development of colour-vision seem to be justified by the results we have cited. First of all, an articulation takes place with reference to colour and non-colour, and this occurs earlier with the long-wave colours than with the short-wave colours. Consider now the developmental series of Winch and Meumann in contrast with the one obtained by Garbini (cf. above p. 287). If we omit the position of orange in Garbini's series, the difference is much less than at first it appears to be. After red there follows in each series a " cold " colour, then a second " warm " and a second " cold " colour—although in inverted order—and finally an " intermediate colour," violet, followed in the series of Winch and Meumann by orange, which occurs earlier in Garbini's series. Since the methods of testing and learning were different in all three cases, one could hardly expect a closer correspondence ; yet in my opinion this much, at least, can be provisionally inferred from these results : After the stage of colour—non-colour, described above, there follows a period in which " warm—cold " and probably also " warm—colourless " and " cold—colourless " configurations arise. This development would account for the confusion of blue and green, the first achievements being configurations which are also characteristic of the intermediate zone of the retina, and like the phenomena of red-green colour-blindness. How intimate the connection with colour-blindness may be, the material at hand is too incomplete to determine.

We may next suppose that a differentiation takes place within the " warm " and " cold " colours, causing the four principal colours, red, yellow, green, and blue to appear. More exactly, we can say that in opposition to the colourless experiences, colour-configurations are

constituted in four directions.[306] Here, too, I have been able to find an analogy in a case of defective vision. The last step would be a differentiation leading to the appearance of the intermediate colours, and although the development is essentially one of maturation, practice influences it very markedly. Thus, the considerable difference in the reactions of Miss Shinn's niece and the Stern children may, in large part, depend upon their respective environments; for the latter children grew up in the stony surroundings of Breslau, whereas Miss Shinn's niece lived in a country house amid the luxuriant landscape of California.

According to our view, the learning of colour-names depends upon the possibility of arousing accurate colour-configurations. The connection between colour-configuration and name has never, perhaps, been so strikingly observed as by Stumpf in the case of his own child. Up to his fourth year this boy (as we shall have occasion to note at the close of the chapter) spoke a language of his own—a language which contained but two colour-names: *ä* and *weich*. "Every colour in contrast with white was called *ä*, and in contrast with black, *weich*; or speaking more generally, the darker of any two colours was *ä* and the lighter *weich*." [307]

To us, the configuration is the primary characteristic, the name of the colour being secondary. But this point of view has been completely reversed by Peters in a work written with great insight upon the basis of certain experimental distinctions. Peters regards the confusions which children make, not only in naming but also in arranging colours, as a result of the influence exerted by their names upon the apprehension and comparison of the colours themselves.[308] He confines himself to the confusion of intermediate and principal colours, blue and violet, red and purple, etc., and deduces five consequences of his thesis which he then attempts to prove experimentally.

(1) Children who know no definite colour names, ought to commit no errors in arrangement; (2) neither should they commit any errors when the correct names are acquired. (3) On the other hand, such children must commit errors whenever the same name is applied to both *principal* and *intermediate* colours. (4) Children who are already able to name the intermediate colours correctly, should make no errors in arrangement. (5) Children who at first commit errors in naming and arranging should correct these errors as soon as they have learned to name the colours correctly.

Peters believes that he has demonstrated all five of these inferences. He concludes, therefore, that the development of colour-perception in older children has nothing to do with sensory functioning, or with its morphological substratum, but depends altogether upon the constitution of the so-called higher intellectual processes of apprehension, reproduction, and thinking with respect to these sensory capacities. Apprehension is not altogether determined by sensation, since a knowledge of the colour-names may, under certain circumstances, be of greater significance than the sensory component; thus, but for the naming of the colour, no errors at all would ever be made. "A child who attaches the same name, blue, to both blue and violet, does not merely apprehend violet as something which looks so-and-so, but also as an object which is called blue. . . . The colour-name which thus influences apprehension—one might speak here of a verbo-perceptive influence—is in both colours the same, and the knowledge of this common term has the obvious effect of setting aside any difference in their appearance, so long as this difference is not too great." [309] We have so often directed our argument against the employment of such concepts as "apprehension," etc., that the reader will at once be able to formulate for himself our objection to this particular interpretation. We have only to consider Peters' experimental results

apart from the distinction he draws between the sensory and the higher intellectual functions—as though these existed separately side by side—in order to indicate their real value.

Let us therefore review these experiments in detail. Backward children were made the subjects of the investigation, and since all possible stages of colour-mastery can be found in these children, they furnish exceptionally good subjects from which to obtain an answer to the question raised. The children tested ranged between 6.10 and 12 years of age, and their mental ages varied from 5 to 9.4 years. In determining mental age, the children were classified in comparison with normal children by means of the Binet-Simon scale of tests. This is not the place to discuss this method of testing, but one should not expect to find by such means anything more than an approximate characterization of mental age. Peters' tests show that a backward child of a certain mental age is not at all the equivalent of a normal child of the same chronological age; for Peters remarks of his backward children that a momentarily successful practice in naming colours lasted only for a brief period of time. (Cf. also the observations on p. 35.)

The investigations themselves were experiments upon the arrangement of coloured samples. A coloured skein of wool was laid before the child, who was then given the task of selecting "all the others that look like this wool here" from a pile of thoroughly mixed skeins consisting of seventeen different nuances; three skeins having been provided for each of six colours.

The child was then taught to name certain colours. Each colour was shown again and again in a constantly changing series and, with the aid of pointing the finger, its name was repeated.

Peters did, indeed, find support for each of the five inferences stated above. Unfortunately, he found only one instance in which the child originally possessed no

definite colour-names, and hence, in accordance with the first inference, made no errors in his arrangement; this child, however, did place some skeins of brighter and less saturated blue along with the blue sample. A boy who possessed an almost perfect understanding of the colour-names—so that he even called violet colours *lilac*—misnamed only the purple, which he called *red*. Yet in the arrangement-experiment this same boy reacted differently when he received a red, than when he received a purple sample; for although he made no errors with the red, afterwards when he was given the purple sample he selected not only all the purples but also the reds. Peters does not make allowance for this striking behaviour. He concludes from the experiment in which a correct arrangement had previously been made with reference to the blue sample, that as soon as the names of the intermediate colours were known there were no errors; but so long as only the names of principal colours were known, errors of arrangement occurred. Peters' second inference, however, is further-reaching than the experimental report warrants; because errors of arrangement occurred only when the intermediate, and *not* when the principal colours were used as samples.[310] A behaviour of this kind was partially duplicated in another experiment. The name red for red and purple tones, and the name blue for both blue and violet tones, were taught to a boy who knew no colour-names, and therefore did not confuse the colours at all. After instruction, this boy placed all the blue and violet tones with the blue sample; with the red sample, however, he placed no purple tones at all, but only the red ones. Unfortunately, no experiment was made with the purple sample as the standard of comparison.

The experiment with a little girl who, among all the colours, named only red and blue correctly, went very prettily (cf. our discussion above on p. 291). With the red sample she placed red, purple, and lilac; and with

the blue sample, blue, violet, and lilac. The name *violet* was then suggested to her, and no more errors were made with the blue or purple samples ; though she repeatedly selected the wrong skeins, either violet or blue, from the pile and after comparing them with the sample laid them back again. Peters thinks that this hesitancy may have had some connection with the child's previous habit of calling violet objects blue ; but the same behaviour was observed in a boy who named only the principal colours correctly, although he committed no errors in his arrangements.

The experimental results which have now been reviewed seem to demonstrate that Peters' theory is at best incomplete. Yet the experiments also indicate the direction in which we must seek an explanation of the discrepancies noted. Let us begin with the data last mentioned where no actual errors in arrangement were made. A wrong skein was often rejected only after it had been selected for comparison with the sample. There are two points of interest here: (1) Why was the wrong colour taken from the pile at all ; and (2) What does this comparison signify ? The second question can be readily answered. When colours are held side by side—the sample and a differently coloured skein—they exist together as members of a configuration which, since the comparison led to rejection, proved to be a configuration involving a *difference*.

Peters has a theory to cover the first question, but we have already seen that his theory fits the facts only in special instances, and not generally. Apart from these special instances, Peters' theory depends upon the soundness of his entire hypothesis. If this be superfluous, we can say that the wrong colour was picked up because such an act was provoked by the quality of the colour itself ; furthermore, the colour was chosen for comparison with the sample-colour because the provocation involved an *index of uncertainty*. One result of name-learning would therefore be that colours acquire

indices of uncertainty; which leads us to the main problem—What takes place in learning of this type? According to Peters, learning is solely a matter of the connection between a sensation and a name. But we have already seen that a connection of this sort is not the primary achievement of any systematic training. The important thing is that the child shall "see the point" of the tests. If a child, to whom blue skeins of wool are named blue, while violet ones are called violet, intends to learn this fact, he must first understand, however incompletely, why colours, which hitherto have borne the same name, should now have different names. This means that in the process of learning the child must acquire a new colour-configuration. He must learn to see something different when blue appears on the background than when violet is there. This is the most natural thing in the world to me—a partially colour-weak person. When a child I could never understand why adults often called "blue" things "lilac." I have since learned why, though rather incompletely; for I now know that a blue can be reddish, and I therefore try to reconstruct the colour as a red. This is often difficult, and sometimes impossible. But if I can lay a blue colour alongside of one that is doubtful, such as violet, my doubt is removed; for in the colour-pair the one which was just now bluish and very "doubtful" becomes strongly reddish, often, indeed, quite purple. When, therefore, a child is taught to give the same names to principal and intermediate colours, whereas previously he had made no use at all of colour-names, the child must learn, for instance, when to say blue, and when to say red. Thus he tries to construct the *same* configuration for blue on a background that he does for violet on a background (and likewise for red and purple). The fact that principal and intermediate colours are not named differently until much later is, from our point of view, a sign that the original form was a configuration of colour upon a gray ground in

which no distinction was made between principal and intermediate colours. Although we are here dealing with a "verbo-perceptive" influence, its effect is quite different from the one Peters refers to.

We can now understand, not only Peters' five inferences, but also the facts which he did not explain. Without going into details, it should be noticed that during instruction the child has figural experiences of colour and ground, and that in the selection which takes place the figures are complicated by heaping the colours together and thus mixing the different strands of wool. This is undoubtedly the principal reason why wrong colours are so often placed beside the test-colour for comparison, leading, finally, to a figure for comparison in which an intermediate colour is contrasted with a principal colour. In the matter of differentiating principal and intermediate colours in incorrect arrangements (cf. p. 295), the following may be said: Psychologically, an arrangement of colours with reference to a red standard is not the same as an arrangement with reference to a purple standard, even though the same name is attached to each standard. Since purple against a background gives the same kind of figure as red against a background, when the relational system has purple as its standard, all reds will belong in this system. Consequently, the figure in which purple is differentiated from red does not come into consideration, because the standard, with reference to its background, already possesses the characteristic of red. On the other hand, when the standard is red, the purple figure can easily arise in opposition to the red; and this opposition may be carried in the memory so that purple will be rejected; again indicating that the principal colours have an outstanding position.

Peters has, in point of fact, demonstrated the influence exerted by names upon the apprehension and comparison of colours, but "apprehension" and "comparison" need not be taken as processes of a "higher"

order that are merely added to a "lower" order of unchanging sensory processes. Instead, both are configurative processes determining the quality of their membership, including the so-called "sensations." In this respect Peters' experiments furnish a valuable support for, as well as a deeper insight into, our own theory.

The recent investigation of colour-name amnesia by Gelb and Goldstein [311] makes it appear that our interpretation of Peters' results is incomplete. According to these authors, language exerts a specific influence upon perception which they term a "categorical behaviour." Accordingly, a colour, for instance, "will be analyzed out of a given connection and taken merely as the representative of a certain colour-category, that is, as a representation of *red, yellow, blue*, etc." This would suggest that a "categorical behaviour" which is something other, and more, than a mere connection between colour and name was operative in Peters' experiments, although we can not tell in what form or in what degree. But this suggestion in no way contradicts our previous discussion of Peters' conclusions; for what we have written is also applicable to the development of such "categories."

The untenability of Peters' hypothesis is indicated by an argument he advances in its support. The influence of knowledge upon perception is often remarked in the case of adults; especially with reference to colour. For instance, a white lying in a shadow does not look black, neither does a gray in full illumination look white, provided one is able to survey the spatial arrangement of each. Hering was the first to call attention to these phenomena, which he termed *memory-colours*, although Hering's theory differs from that of Peters. Katz,[312] who has made a thorough investigation of these phenomena, found that the apparent whiteness of an achromatic tone maintains a relative independence of the amount of light reflected from its surface into

the eye, even when no knowledge at all is given concerning the "actual" nature of the colour. On the other hand, he found that this "reference to the illumination," this relative "constancy of colour," is bound up with the fact that the colour appears as the *colour of an object*, and not merely as an extended coloration, such, for instance, as that of the blue heavens. This discovery has been substantiated, and given greater precision, by Gelb's observations upon pathological cases.[313] Katz, however, regarded this "constancy of colour" as an effect of memory, that is, as a product of experience.

By means of choice-experiments, of the kind already known to us, Köhler[314] has been able to show that "constancy of colour" is indicated by chimpanzees, and even by hens. The hens experimented upon varied in age from seven to fifteen months. One-half of them were trained to eat from a white surface, and the other half from a black, the two surfaces being placed side by side in the same illumination. It was found that the influence of this training remained without alteration, even when the white surface was shaded to such an extent that it reflected less light than the black surface—the black surface in some cases being objectively 12.4 times brighter than the white. Not only is knowledge or any "verbo-perceptive" influence excluded, but likewise any effect of experience whatsoever; for, if the word "experience" is to have any meaning at all in the explanation of human behaviour based on perception, it is certainly not applicable to the experience of a seven-months-old hen—and the same experiments can be made with even younger fowls.

Since in this "constancy of colour" the bearing of one colour upon another is always involved, we can not be accused of anticipating the development of our theory if we again apply the operation of configural functions to our explanation; especially when we consider that

Köhler's selective trainings all depended upon such configurations. Consequently, the facts Peters adduces in support of his theory, as well as his experiments, both lead us back to our own theory of the development of colour-vision. How very young children would behave under similar conditions has not yet been investigated, but the problem is one well worth undertaking.

§ 6—*Continuation : Spatial Factors*

We shall now consider a few of the more important problems involved in the development of visual space-perception. In the beginning the infant's field of vision, considered as an area within which visible objects arouse reactions, is very limited. At first the child sees only what lies directly before him ; objects appearing but slightly to one side, above or below, are practically non-existent. Similarly, visual depth is very slight. Stern calls this perceptual limitation *near-space*, and reckons it approximately as a half-sphere about the head with a radius of perhaps a third of a metre. Whatever lies beyond this range is not seen with any specific quality, although it may contribute to the general background of visual experience. This limit of a third of a metre is not inflexible, however, but depends upon the kind of object seen ; indeed, a general variability of this sort obtains throughout the entire field of vision. Thus bright objects can be perceived at greater distances from the centre than dark objects, whether with reference to height, breadth, or depth. Compayré reports on this point as follows: "Place a lighted candle two or three metres from a child fifteen or twenty days old ; he will look at it fixedly ; if you place it three, four, or five metres from him, it will become evident that the child has lost sight of the light, and you will be sure from the uncertainty of his glances that he no longer perceives anything."

As regards the absolute magnitude of the distance, reports of observers vary considerably.[315]

Attempts have been made to explain these facts on the ground that a restricted field of vision depends upon a later development in functional capacity of the peripheral as compared with the central portion of the retina; while inability to apprehend visual depth has been attributed to an original incompleteness of eye-movements, especially those of accommodation and convergence. But this cannot be an adequate statement of the case; because in certain ways these characteristics of visual space, as found in early childhood, recur again in adult life. The peripheral portions of the field of vision, as well as the remote distances of visual space, are always at a disadvantage in comparison with the nearer regions; and this is true for perceptions of colour as well as for those of form and magnitude. Analogous to the results obtained with children, the degree of this disadvantage depends upon the nature of the object selected for the test.[316] This latter circumstance, in particular, contradicts the all too simple nature of the explanation which has been offered. We must think of development in terms of a process of maturation in the course of which certain regions of the nervous system attain the capacity of forming fixed configurations which at first they do not possess; this process of maturation being dependent upon functional employment. From numerous pathological observations we know that even an adult is able to develop such an ability through practice, when the ability is needful to him. Biological importance attaches at first only to what is near at hand; and to be able to see at great distances is for most living beings of no importance whatever. That a dog, for instance, should be able to see the mountains which enclose a valley seems to me, from personal observation, highly improbable.

A connection also exists between the extension of

the field of visual space and still another of its properties. To us adults, the "apparent magnitude" of an object—that is, how large a thing looks—is relatively independent of the actual magnitude of its retinal image. When a man removes himself from a distance of one metre to a distance of four metres from us, he does not suddenly shrink into one-quarter the size of his previous appearance, even though the retinal image must have undergone diminution to this extent. As a matter of fact, we see no change of magnitude at all. Thus, within a certain distance we never confuse a small object near at hand with a large object farther off. Yet this independence of retinal magnitude is not absolute; for when I find myself at a considerable distance from the man, he suddenly appears very small indeed. A village seen from a mountain top may look like a toy which came out of a box, and even a very high mountain peak when seen from another peak at a great distance may look like a minute point. On the other hand, there is a certain adequate distance, a zone, as it were, within which the "actual magnitude" of an object is best apprehended, and this distance is different in apprehending a thimble than it is in apprehending a man, and is again different in apprehending a mountain.[317]

These phenomena have usually been explained as the results of experience in the way indicated by Helmholtz. Stern, it is true, no longer speaks of an involved association between the impression of distance and that of magnitude; but he still accepts an empiristic theory based upon visual and tactual impressions, and Bühler likewise believes that the relative independence of apparent magnitude from the retinal image "must first be acquired and practised by the child."[318]

Until quite recently our knowledge of the constancy of size for children rested entirely upon the casual recollections of adults. Helmholtz reports an undated remembrance of childhood, when human beings seen

on a church-tower in Potsdam looked to him like dolls. I can also recall a very similar experience. On the Victory-column in Berlin cannon are placed at different heights, and I well remember that I could scarcely believe my father, with whom I often passed by this column, when he told me they were all cannon; for while the lower ones did appear like short rifles, the higher ones seemed like small pistols. Although they no longer look that way to me, the upper ones still seem smaller than the lower, and no amount of knowledge has sufficed to alter this sensory impression, which is in direct contradiction with Helmholtz's explanation from experience, or the association of sensations with ideas and judgments.

The first experimental attack upon this problem was made, not with children, but with chimpanzees.[319] Köhler trained his animals to choose the larger of two boxes having front-boards of different size, the boxes being at like distances from the animal. The larger box was then so far removed that the retinal image of its front became smaller than that of the smaller box. All necessary precautions were taken into consideration, and yet the effect of training persisted. Even the behaviour of a four-year-old chimpanzee indicated the constancy of apparent magnitude within a certain zone of distance; which shows that the usual hypothesis referring the constancy of selection to experience is highly improbable, if not impossible.

It has become quite impossible since Götz has shown that chicks, three months old, trained to peck only at the bigger of two grains, will continue to do so when the two grains are placed at such different distances from the animal that the larger one appears under a much smaller angle than the smaller one (area of the larger 1/30 of the area of the smaller).

Also, quite recently, Helene Frank has applied Köhler's method to children varying in age from eleven months to seven years. Her results are like-

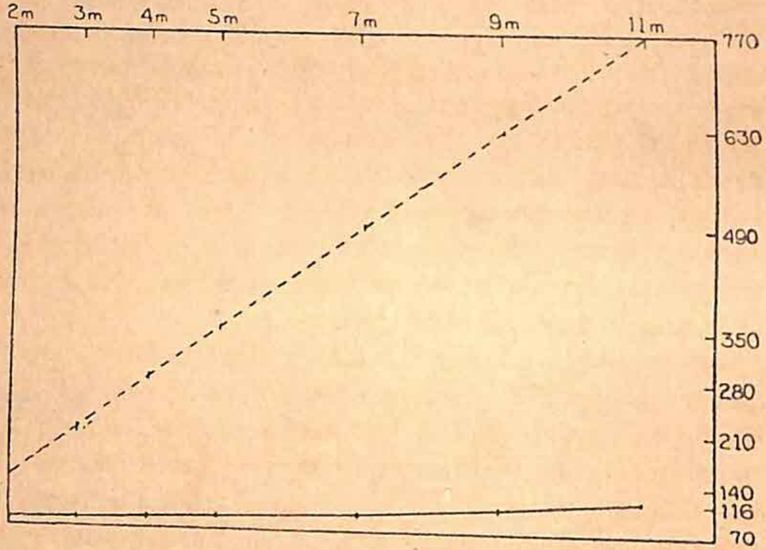
wise positive. An infant of eleven months, for instance, which had been trained to select the larger of two boxes standing side by side, continued in her choice when the larger box was placed so much further away from the child than the smaller one that the ratio of the areas of the two retinal images was 6.2 : 100. This result is incompatible with an "empiristic" explanation of the constancy of size.

A new step was taken by Beyrl who tried to measure the degree of constancy by determining how large an object must be at various distances in order to be judged equal to one at 1 metre-distance. Working with children whose ages varied from two to ten years, he found a gradual development. However, as Mrs. Frank has pointed out in a later paper, in which she reports new experiments, the method employed by Beyrl was such that it lowered the degree of constancy for the younger children (up to four years of age). Consequently, the achievement of the younger children appeared to be less than it is under more favourable conditions, and the development of the pure constancy of size is less marked than Beyrl believes. But even in his experiments the constancy was remarkable as is shown by the following diagram which we reproduce from his paper. On the abscissa are plotted the distances of the boxes from the observer, and on the ordinate the size of the edge of the boxes. The observations were made by two-year-old children. The dotted line indicates the change in size necessary to maintain a definite retinal image. The continuous line indicates the actual size of the box edges which appeared to be the same at different distances. (See Fig. 14.)

The diagram indicates clearly that the perception of magnitude, even of two-year-old children, is very near to absolute constancy within the investigated range of distances. Another significant result of Beyrl was that the degree of constancy depends upon the object chosen, being greater for three-dimensioned

objects (boxes) than for two-dimensioned ones (discs).[320]

Our refusal to accept an "empiristic" explanation of the constancy of size is in harmony with many other facts concerning apparent magnitude; such, for instance, as its relation to clearness, to impressiveness, to the configuration of what is seen—that the smaller the apparent magnitude, corresponding *ceteris paribus* to a definite retinal image, the greater the clearness attaching to



[*After Beyrl.*

FIG. 14.

it—and the zenith-horizon illusion of the moon.[321] All these facts indicate that constancy of size is a direct result of external and internal causes.

The development of this capacity is in all likelihood more a matter of maturation than of learning; although the process is obviously of such an order that it can not go on independent of the employment of the organs concerned.[322] That is why I have suggested above that a connection must exist between the development of an apparent constancy of magnitude, and the development of spatial extension. Even at a relatively

late period of life this development is not yet at an end, as was demonstrated both by the observations of Helmholtz and myself; mine certainly extended into the sixth, if not into the seventh year of my life. These observations, however, do not mean that constancy of magnitude may not already have been established for lesser distances than those referred to.

The following observation of Stern does not aid us at all with our problem. Once when the baby was eight months old, while waiting for his bottle, he was shown, by way of a joke, a doll's bottle about one-fifteenth the usual size: "He showed the greatest excitement, and snatched at the bottle as if it were really the right one." As Stern points out, this behaviour demonstrates how small a part size actually plays in the recognition of things during this period of life; but it does not indicate, as Stern also infers, that a constancy of magnitude, in the sense in which we have employed the term,[323] must have been lacking.

An hypothesis based upon experiments with eidetic images (cf. pp. 259 f.), which has been advanced by Paula Busse to explain the as yet uninvestigated course of development in the constancy of magnitude, seems to me likewise untenable. Her idea is that the eidetic image of an object when seen at close range ought to fuse with the perceptual complex of the same object when it is more distant, thus maintaining the constancy of magnitude.[324] There is nothing to question in her observations, which are interesting and important enough in themselves, but just how they relate to the constancy of magnitude and its development is a matter that must be investigated in greater detail. For instance, in a demonstration made by Jaensch before a scientific gathering at Nauheim in 1920, a remote object was so influenced by the constitution of the eidetic image that it appeared to be enlarged beyond its actual limits, previously determined by the points of a compass. In a case like this, the matter is extremely com-

plicated; the factors in the configuration upon which the apparent magnitude depends may exercise a different influence upon the eidetic image than they do upon either the perceptual, or the after-images.[325] Nor is there any reason to suppose that one of these influences is more original than another, so that the others must be derived from it.

The perception of *form* confirms the suitability of our general principle of explanation. We have previously referred to the fact (cf. above p. 148) that it is not the simplest geometrical forms, but those biologically most important, which are first evident in infantile perception. This fact is demonstrable in a number of different ways. We know from the investigations of Lohnert and Lenk that with adults the sensitivity for size-differences increases with the degree of articulation in the figures compared. We also know from Stern's observations on two little girls, aged 1.9 and 3.6, that despite an indifference to absolute magnitude (see above p. 307), a child is able to recognize subtle relations between magnitudes. Recently, Dora Musold, working under Volkelt's direction, has tried to find out whether the general result obtained in Lohnert's and Lenk's experiments applies also to children.[326] She measured the discrimination of sizes with three groups of subjects, the first being composed of children between three and six years of age, the second of school-children, and the third of adults. She employed four kinds of test-objects, namely (1) the sphere, (2) the surface-circle, (3) the contour-circle, and (4) the straight line. Her results were that in all three groups of subjects sensitivity was greatest for the sphere, and least for the straight line. The difference between the sensitivity for the sphere and for the straight line was least in school-children, almost as little in adults, and of quite a different order of magnitude in young children. As a matter of fact young children, although vastly inferior to adults in the comparison of straight and circular lines, were

practically their peers in the comparison of spheres. Were the straight line genetically the simplest figure the result sought, of course, to have been just the reverse.

Other evidence is afforded by the observations of Miss Shinn. From her 25th day forward, Miss Shinn's niece took an interest in human faces, which in her second quarter-year she was able to distinguish as familiar and unfamiliar. A child can be taught "simple figures" only at a much later time. Miss Shinn, to whom we owe a number of good observations on this subject, was able to impress her niece with the printed letter *o* in the beginning of her twelfth month. From her 343rd day the child pointed out the *o* correctly, while in the thirteenth month her behaviour showed a marked independence of the absolute magnitude of the letter. On her 382nd day the child found an *o* printed in small type; and thereafter would occasionally confuse *o*'s with *c*'s. This behaviour is very instructive, because sensory-wise *c* and *o* are quite different, but as a figure a *c* may be taken for an "incomplete" *o*. At the end of her 21st month, the names of the forms which the child had learned from a toy consisting of small and variously shaped pieces of cardboard, were for the first time applied to things in her environment. Thus, for instance, the folded edge of a man's collar was called a triangle. This should not be understood to mean that the collar-edge simply recalled the name *triangle* on account of its similarity with the triangular cardboard, but rather that the triangular configuration which was acquired in the use of the toy now entered into the perception of a man's collar. That is to say, the progress which the child had made was not merely in naming, but essentially in perceiving.

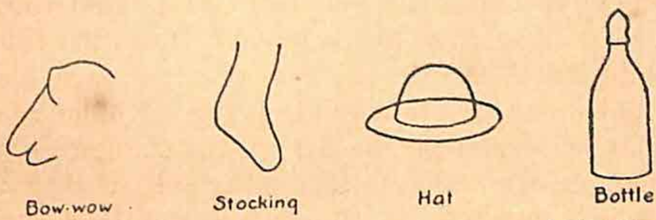
At the end of her 22nd month, after the child had acquired a remarkable facility in dealing with and in recognizing plane figures, experiments were performed with simple geometrical solids. These solids gave the child considerable difficulty, especially in learning to

use the word *cube*, instead of which she always said *square;* although she learned very readily to employ the word *ball* for a *sphere*. This difference also indicates a peculiarity of perception. From the start, the figure of a cube is very closely related to the figure of a square, whereas a ball is evidently something quite different from a flat circle.

In consideration of what has previously been said, these achievements are relatively late; moreover, children are able to recognize pictures of persons at a much earlier date. They also take pictures for actual things; fear, for instance, was shown by Miss Shinn's niece on her 293rd day when the picture of a cat was placed before her. The behaviour of a child towards pictures is also like his behaviour towards things; the child will look into the eyes of a portrait, just as he would into those of a living being. Köhler reports a similar behaviour on the part of his chimpanzees, all of whom recognized apes in photographs (size $8 \times 10\frac{1}{2}$ cm.), which depicted the animals in magnitudes varying from 4 to 8 cm. "When it was Sultan's turn," writes Köhler, "and I showed him his own likeness, he examined it keenly for a while, and then suddenly raised his arm and stretched out his hand towards the picture, in the specific gesture of friendly greeting I have already described—palm inward." Furthermore, Köhler was able to demonstrate, with the aid of special choice-training experiments, that apes are quite able to recognize an object in a photograph.[327] Miss Shinn's niece, in her tenth month, recognized large portraits as those of human beings, and she recognized individuals, such as "mother," "father," etc., as early as the beginning of her second year. Even small photographs were recognized, and the father was picked out among a group of other persons. A child of nine months takes pleasure in his picture-book, and really knows the pictures. According to Stern, the crude outline at first determines recognition, while an equally crude filling-in

of the surface holds the second place in his interest ; the finer details, however, attain importance very gradually. Stern tested the significance of the outline by a neat method which he calls the method of "formation." "A sketch is made before the child's eyes, and stopped the moment the child gives the drawing a name." [328] Some of these tests are here reproduced, and crude as the drawings are, they were recognized at the age 1.10 (Fig. 15).

These experiments prove that the figures of early infantile perception may be readily aroused, though they are still very vague, and possess little in the way



[After Stern.

FIG. 15.

of internal structure. The internal structure, however, becomes constantly more definite as the child develops. And hence, figures which a child recognizes at an early age are sometimes not recognized later. Hilda Stern, for instance, who recognized the bottle in the above figure at the age of 1.10, could not recognize the same picture two and one-half years later. Binet also experimented upon a little girl of 1.9 with simple outline drawings. From his results we may note the following. Expressions of smiling and weeping, and the direction of the gaze, were recognized in faces where the achievement would have been considerable if one were to think of it in terms of the geometry of drawing, since the differences of expression were determined by fine differences of internal structure.[329] This result would seem to contradict Stern's conclusion as to the primary

significance of the outline; though the facts agree very well with data reported concerning the recognition of photographs, where Stern himself calls attention to this contradiction. Binet's experiments also contradict the otherwise quite improbable explanation offered by Stern, who states that the recollection a child has of his parents must be much more detailed than his recollections of other objects. Since the child can distinguish fine nuances of expression in quite unfamiliar faces, this fact can only be explained in agreement with our previous results when we regard expression as a part of these quite early phenomena, so that what the child recognizes in the picture is the expression itself rather than the formation of a surface. Indeed, what the child recognizes in the face of his father is not the colour, the size, the distance between the eyes, the form of the nose, mouth, chin, etc., but those essential characteristics that enable us to differentiate a good photograph from a poor, though geometrically correct, one—that is, those properties of a picture for which we have no special name in our language.

Another of Binet's results is not less interesting. When presented severally, the child fails to recognize the isolated parts of objects which he would recognize without hesitation were they exhibited in their proper relations. Thus an ear, a mouth, or a finger was not recognized in Binet's tests, even when the test was repeated nearly three years later (at the age of 4.4); which shows very clearly that quite different phenomena may correspond to the same objectively given thing (the outline of an ear, for example), according to its context. To employ two expressions coined by Wertheimer, a familiar "whole-part" or a completely unfamiliar "part-whole" may correspond to the same objectively given thing. Thus Binet's results do not run counter to those of Guillaume, who succeeded in an experiment upon a child of one year and ten months, in which he drew a rough sketch of a human face, and

allowed the child to indicate where a part, such as an eye or an ear, was missing.[330]

What these examples indicate may be expressed by saying that phenomenally, to a child, a man is not made up of his members, but the members belong to the man. Close ethnological parallels of these facts can be found. Thus, in many languages it is impossible to say merely " hand," because hand is always the hand of a particular person. If, for example, an Indian were to find an amputated arm, he could not say : " I have found an arm," but he must say, " I have found of some one his arm." [331] On the other hand, these facts are supplemented by the observations of Van Gennep, who found that his five-year old daughter was able to reproduce relatively complicated designs which meant something to her, more accurately than she could the technically and geometrically simpler printed letters which she did not know.[332]

Stern has pointed out a further peculiarity of infantile perception ; [333] namely, that to a child a form is much more independent of its *absolute spatial position* than it is to us adults. Children often look at their picture-books upside down without being in the least disturbed, and investigators have shown that pictures turned at an angle of ninety, or even one-hundred-and-eighty, degrees are as easily recognized as those in a normal position. This peculiarity continues for a long time. Even at the beginning of the school-period it may be noticed that many children copy the letters given them, not only in the right position, but in all possible positions ; as, for instance, in mirror-writing, or upside down. Teachers who, at my request, have made observations upon this subject, have reported that certain children can read mirror-writing at first just as well as they can ordinary writing ; which shows the difference between children and adults ; for an adult finds it no easy task to read mirror-writing. Originally, then, a figure is in a high degree independent of its position,

whereas for adults the absolute orientation of the figure is a very powerful factor. Right and left, above and below, become characteristic properties of the different members of the configuration; and consequently of the total-form. A closer investigation of the development of this positional factor in children's perceptions would certainly prove a stimulating and a valuable undertaking. One might suppose, for instance, that the well-known over-estimation of a square standing on a point, as compared with one of the same size lying on its side (Fig. 16), would not exist for children whose forms are as yet independent of spatial positions.

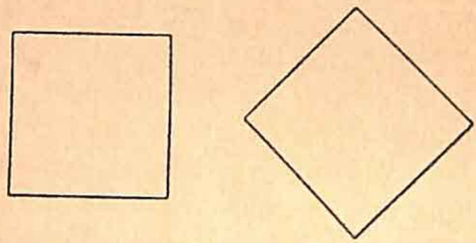


FIG. 16.

In this connection one of Oetjen's results is noteworthy, namely, that turning the reading-page 90° occasioned less difficulty for boys between the ages of nine and thirteen and one-half years than it did for adults.[334] Interesting, too, is the following ethnological parallel given by Pechuël-Loesche. In studying the natives of Loango (Bantu negroes) he found that they "could see and comprehend pictures with which they were acquainted quite as well when they were wrong-side up as when they were right-side up. Likewise, those who could read did so quite as well when the print was reversed as when it was in normal position."[335]

The independence of figure and spatial position may be connected with the independence of figure and magnitude which has already been mentioned. The varying possibilities of formulating the perceptual world of an adult, according to form, magnitude, position and

colour, all entering into one configuration which is determined in many ways, are to a child still more or less independent of one another. But we must not forget that even with us the connection is not so close as it might seem to be from a purely rational and logical consideration; for it would be too much to suppose that we adults complete all the configurations named, simultaneously and with the same degree of distinctness. On the contrary, we see in general much less in things than we might; for instance, it is quite possible to see something large and dark, without being able to specify its form or indicate its colour. To give a common, everyday example, one sees a man with very friendly little eyes, and yet has not the faintest notion whether they are blue or brown.

As a final problem in the perception of form, things in our environment may be seen from very different points of view, and in very different aspects, so that the same thing may be reflected upon the retina in a multiplicity of ways; yet just as in the case of colour and magnitude, the actual phenomenon as it is given to the naïve individual fails to follow these changes; instead a certain thing always appears with the same configurative qualities that are most characteristic of it. When, as is usually the case, I see a chair in such a position that none of the corners of its seat is projected upon my retinæ as a right angle, my perceptual phenomenon of the seat is nevertheless a rectangle. One finds this kind of perception in experiments with any sort of figure with which the observer is unacquainted before the experiments are made. Under these conditions one also finds that the perceptual phenomenon does not follow the "aspect," but shows a marked tendency to be seen as it actually is; that is, in a manner corresponding to its orthogonal appearance, with an orientation at right angles to the line of regard. This effect, like that of the constancy of colour and magnitude, is of enormous importance in the con-

struction of our perception of the world. We shall refer to it as a *constancy of form*. Bühler finds here an analogy, which I think to be correct, between the perceptual constancy of form and the nature of our concepts.[336]

From children's drawings we can infer that this constancy of form becomes the child's mode of perception at a very early age. If a child is called upon to draw a cube from memory, or from a model, or even from a plan in perspective (according to Katz), what he actually draws, as a rule, is a number of connected squares. Many adults, too—as for instance the author and his wife—if called upon to draw things which are not quite easy, like a chair, will do exactly the same thing; a fact which has been demonstrated experimentally by J. Wittmann.[337] Again and again one tries to draw the back and seat of a chair as rectangles, and when the drawing fails to look right, one resorts to all manner of intellectual tricks; because to perceive only a certain aspect of any thing is a task which can be achieved by many persons only with the greatest effort and practice. It is not the same with those who possess some talent in drawing; for they learn with relative ease, some perhaps even without external aid; yet a correct apprehension of the *appearance* of a thing is certainly neither a natural nor an original propensity. At first each thing has actually but one phenomenal appearance or, perhaps, in some cases a small number of appearances; and these succeed in maintaining themselves despite all changes of perspective. This prominent aspect of a thing is both "simple," and perspicuous.[338] The question then arises: How does it happen that this simple form is maintained even when the objective conditions do not favour its arousal? One has been accustomed to call upon memory for an explanation; thus Bühler states that a child is unable to extricate its immediate impressions of form from the influence of previous experience.[339] That would

seem to mean that without previous experience the child ought to see a thing exactly as it appears, and not *orthoscopically*, as Bühler calls it. Wittmann, too, thinks that without experience we apprehend, first of all, the actual objective appearance.[340] I would suggest however, that the explanation is not given by memory, or at least not primarily, but involves the laws of configuration, which indicate that certain forms are favoured from the start and that these forms are at the same time geometrically "simple" and physically outstanding.[341] Only in this way can one actually explain orthoscopic forms, because the instance in which a view in perspective furnishes an exact correspondence between just one face of the body and its orthoscopic appearance is so very infrequent that, strictly speaking, its probability is zero—one favourable instance as compared with an infinite number of unfavourable instances. The presumption then would be that an object is fully apprehended—that is to say, instead of remaining chaotic, it will arouse a phenomenal configuration—only when it happens to be seen in a way that favours orthoscopy. Thus a cube will be apprehended as such only when one happens to stand more or less parallel with its front, and will not be apprehended as a cube when one corner happens to be slanted forwards. After the orthoscopic configuration has once been aroused, however, it maintains itself with reference to quite different aspects, in which the task of construction is more difficult; but even then the case is not one of simple memory; for the objective aspect must also be reckoned with, especially when the appearance varies considerably from the orthoscopic view. When this variation occurs, the object itself still exerts an influence upon the phenomenal configuration, so that either the orthoscopic form appears in an oblique position, or else a new form arises which stands between the orthoscopic and the perspective appearance. The constancy of form, therefore, just as

in the case of the constancy of magnitude, is not at all absolute.

Since the constancy of form finds an oft-recurring example in the maintenance of the right angle, this instance has been seized by Wertheimer in support of our theory as against an "empiristic" explanation of the facts.[342] The right angle is a favoured form. If one attributes this fact to "experience"—to the frequency with which right angles occur—one substitutes the real object, unwittingly, for the visual stimulus, or for the retinal image. For because of perspective right angles are rarely given as retinal images. Consequently, we can not explain this favoured form on the ground that it is often seen; on the contrary, we must refer the frequency of our perception of right angles to the fact that the form is favoured.

In the investigation of perception we have met with the same kind of functions in the constancy of colour, magnitude, and form. In all three we have rejected an explanation based on individual experience in the sense that experience means either the formation of new connections, or determinations having recourse to "apprehension" and judgment. We have found, instead, certain laws of configural functions developing on the one hand through mere maturation—though not, to be sure, without stimulation—and on the other hand being recast, or newly created. These processes of recasting and creation may be called experiences, but experience in this sense becomes a concept which transcends the dispute over Empiricism and Nativism.[343] To an adult, a configural function, in its phenomenal aspect, is a *perceptual* experience in its own right; for it is neither a mere judgment, nor a mere apprehension of sensations. The development of these configurations can not be conceived as a simple combination of sensations, or as the outward manifestation of a juxtaposition of repeated sensations. On the contrary, we must either think of the configural function as a process which alters, refines,

recentres, and enriches the configuration throughout its entire make-up—a procedure in which maturation participates very largely—or else we must regard it as the arousal of an entirely new configuration.

It is hardly necessary to point out that this discussion of perception is incomplete as regards the numerous shapes actually perceived in our environment. Our phenomenal world consists, not of lines and surfaces having various depths and shapes, but of *things*. We are surrounded by tables and chairs, hotels and post-offices, street-cars and automobiles, animals and men. This fact would seem to favour an empiristic explanation; for how can one know that a certain building is a post-office without being told? In order to understand all that is here involved, one must go further back. Then it will appear that the matter of acquiring knowledge is not a mere accretion of "experiences." Even before I am told that a certain building is a post-office, I perceive it to be a building. In other words, it already has a meaning. But how does it get this meaning, or any meaning at all upon which this meaning rests? The usual assumption is that objects in the first place are merely shaped lines and surfaces to which meaning is added by "experience." But the notion of primitive consciousness which we have previously developed is incompatible with this assumption. If, as we have remarked, an object appears to be attractive or repulsive before it appears to be black or blue, circular or square, then we can not begin with its purely sensory or geometrical properties as original data. The fact is that the meaning of an object is there in the very beginning as a relation which the object bears to the action of the percipient. Since all meanings are not of this simple type, we must distinguish between original and acquired meanings. But how are meanings acquired? The nature of the original meanings will supply an answer to this question. In separating perception from action we

have lost sight of the fundamental fact that, just as action is directed by perception, so perception is influenced by the course of action in which it occurs. In the discussion of Köhler's experiments we pointed out that to an ape a box might be either a means of elevation, a place to lie upon, or an obstacle to be set aside. Only in action can a part of the perceptual field emerge from its ground and acquire the special character of a figure. Furthermore, in action a figure of one kind can be transformed into another kind. For this reason, Ogden defines the term perception as "any experienced circuit of events," a total process involving both a receptive and an effective side.[344]

The acquisition of meaning is a process whereby parts of the perceptual field change their aspects whenever they are included in the activity of the moment. Either they are forced out of the "ground" and become distinct figures, or one figure is transformed into another. In either case, as they become members of the activity at hand, they acquire new aspects with reference to this activity. Since every new aspect gives rise to some new activity, the process of acquiring new meanings is endless. While this process is indeed one of "experience," it is not the blind mechanical kind of experience indicated by the usual "empiristic" theory. Concerning the details of this process of acquiring meanings we know very little; but the facts could be profitably studied by appropriate experiments with infants and young children.[345]

§ 7—*Sensori-Motor Learning: the First Achievements of Training and Intelligence*

Since we have studied the problem of achievement in the previous chapter, we need here consider only a few examples selected from infantile development. As an instance of sensori-motor learning we have already referred to the maxim that a "burnt child shuns the

fire." In this case the matter seems to be much simpler than it is in cases where a positive achievement must be attained; for here one might suppose it possible to explain the result without reference to the problem of achievement. But as a matter of fact that is not true, for this example is only a significant representative of many infantile accomplishments, all of which must be treated exactly as we have treated other instances of learning; a conclusion which becomes apparent as soon as we consider that a burnt moth does *not* shun the fire. What we have called a "first achievement" is here an understanding that pain comes from fire, and that the flame which was at first so attractive and so desirable may become, through painful experience, something "dangerous" and "to be avoided." A mere connection between sense-impression and reaction, or even the destruction of any such original connection, would be an insufficient hypothesis; because a constructive achievement is necessary, however little it may seem to demand of the individual. Consequently, if a child running about in a state of excitement should by chance get burned, he would learn nothing from his painful experience; because, without the participation of attention, learning does not take place. The chief function of pain in these experiences is to arouse attention and thus furnish favourable conditions for the construction of a new configuration. The withdrawal of the burned hand is naturally reflexive, but what is learned is not to withdraw the hand, but to avoid fire; and in a state of inattention nothing at all is learned.

This interpretation of the facts is also confirmed by the experiments Watson has made upon the acquisition of this reaction. For a long time (from the 150th to the 164th day of life) touching the flame, which caused a reflexive flexion of the fingers and a withdrawal of the hand, had no effect of teaching the child to avoid the flame. On the 178th day the reaction was for the first

time modified, and seemed to be distinctly inhibited; but only on the 220th day was the transformation fully accomplished. Then, instead of grasping for the candle, the child slapped at it, and after this new reaction had appeared, the child reached for the flame but a single time.[346]

C. L. Hull describes a similar experiment which he performed with a little girl, aged sixteen months. In this case the transformation took place rapidly. When the flame was first shown to the child, she extinguished it with her hands. When the candle was re-lighted, she shrank away from it with a characteristic gesture and facial expression. This sudden transformation was not limited to the candle-flame; for on the following day she refused to touch a glowing electric-light bulb (8 c.p.) with which she had been accustomed to play, and instead reacted to it with the same gesture and facial expression with which she had reacted to the candle-flame. As soon as the current was turned off she reached as usual for the now dark though still warm bulb.[347]

It is in this way that we must understand the simplest achievements of learning by animals; as, for example, the previously mentioned chick which learned to avoid pecking at evil-tasting cinnabar caterpillars. K. Lewin in his war experiences has vividly described how things undergo a quite analogous change in us adults;[348] how, for instance, the "homogeneous" landscape becomes "limited" and "directed" as one approaches the firing-line; and how a transformation again occurs as one leaves a position behind him, so that suddenly what was a "position" now becomes a mere acre of ground. These are analogies of the process as it presents itself in a most primitive form. In the initial stages of its development the child learns a tremendous amount; much, indeed, at the level Bühler calls training or "drill," though from our point of view we must always presuppose a certain degree of understand-

ing. This statement is also true of accomplishments for which one might perhaps more appropriately reserve the term "training," since they are essentially meaningless to the child. We refer here to types of behaviour instigated chiefly by adults for their own amusement. For instance, one asks a child to do something, or to "say please," or to "tell how big you are," etc. Configurations of this sort are quite vague. A certain child, who had been trained to respond to the sentence: "Bring the butter," by fetching the dish, did so at the age of 1.4 when the father said, "That's a butterball." A still younger child of from six to eight months, who had learned to turn his head in response to the question, "Wo ist das Fenster?" made the same seeking-movements when the question, "où est la fenêtre?" was asked in a similar tone of voice.[349] It is not the total sensory complex with its complete membership which constitutes these configurations, but only a significant accent, or perhaps the vague total form.

Problems, however, soon arise in the everyday life of the child similar to those which Köhler's chimpanzees were called upon to solve. One may therefore inquire when and how the first actual achievements of intelligence arise. Franken[350] has tried the method he used with dogs on two children, aged 2.5 and 3.1, and Köhler himself has reported a few observations made upon young children in experiments like his tests of apes. Bühler has likewise followed with experiments of this order. Bühler began his tests when his child was nine months old, by a clever employment of the child's playful grasping.[351] The infant sat upright in his bed and grasped at everything within reach, in order that he might bring it to his mouth. The behaviour of grasping was then made systematically more difficult. A piece of rusk was placed slightly out of reach, with a string attached to it which came within reaching distance. In another experiment an ivory ring with which the child was accustomed to

play was placed over an upright rod about as large as one's finger, from which the child had to lift it. The principles employed in these experiments are already familiar from the description given of Köhler's procedure. At the beginning of his ninth month the child was unable to make use of a string connected with an object; instead he always "stretched his arm directly towards the biscuit without observing the string. If, by chance, the string was grasped in the hand, it was either let go or pushed aside. Only in two sittings did he appear to comprehend the connection, but this enabled him to perform numerous correct solutions promptly one after the other. I still think that the child did comprehend the situation on these two occasions, though at the next sitting all had been forgotten." Not until the end of the tenth month did the child really "comprehend" the situation well enough so that the string might lead in any direction, and still he would grasp it and fetch the biscuit. Bühler was able to exclude the possibility of this behaviour being an accidental achievement. The result, which has also been obtained by Peiser and Volkelt, is of interest in several respects. We may call particular attention to the *anticipation* involved in the first two instances of success; an example of which has already been noted in connection with the investigation of apes (cf. p. 213). Similar anticipations have been frequently observed in cases of "sensori-motor" development, and we have pointed out the theoretical significance of this concept (cf. above p. 279).

In harmony with Köhler's results, the child found the removal of a ring from a peg a much more difficult task. The act was not successful until the middle of the second year, but the comprehension of the act was then so complete that a key was immediately taken from a nail, and a hat from a cane.

Köhler performed the detour-experiment with a little girl of 1.3 who had been walking alone for only a few

weeks. The child was placed at the end of a blind alley two metres long and a metre and a-half wide. Beyond the restraining partition lay an attractive object, in plain sight but not within her immediate reach. "First she pushed towards the object, i.e., against the partition, then looked round slowly, let her eyes run along the blind alley, suddenly laughed joyfully, and in one movement was off on a trot round the corner to the objective." [352]

With children, as well as with chimpanzees, it is much more difficult to make a detour with the aid of a tool. Köhler used the detour-board (in the normal position—the open side of the enclosure being at the greatest distance from the child—cf. above p. 205) in an experiment with a boy aged 2.1, who had shown that he could readily make detours in his bodily movements. But the boy, who was of average intelligence, was unsuccessful in his performance with a stick. Like the chimpanzees under similar conditions, he gave vent to his desire for the unattainable object by throwing both his stick and his belt at it.[353]

Köhler also found a correspondence between the behaviour of young children and chimpanzees with respect to building operations and the handling of a coiled rope (cf. p. 216). In building, children have at first the same difficulty chimpanzees have in putting one thing upon another, and they fumble about in the most curious manner. At the close of the third year, however, a child will have learned to comprehend the simplest of these achievements, whereas apes make scarcely any real progress even after abundant practice. Since the correct manipulation of a rope may be attributed to a visual achievement, the awkwardness of children, which can be observed up to the fourth year, and even later, may therefore depend in part upon the fact that a wound-up rope does not yet constitute an adequate visual configuration for them.

A pretty incidental observation made by Preyer

furnishes from child-life an analogy to the "employment of a box" by apes. Preyer's seventeen-months-old child was unable to reach his playthings from a high cupboard, so "he ran about, brought a traveling bag, got upon it, and took what he wanted." [354]

It is to be hoped that these methods of investigation, which have now been perfected in principle, will be systematically employed as a means of penetrating more deeply into the development of infantile achievements.

§ 8—*Continuation: the Problem of Imitation*

For the most part a child's achievements are acquired, not in the artificial isolation of an experiment, but in connection with an environment which already dominates the achievements themselves. It is here that we meet with the important problem of *imitation*. Few questions have been so much disputed as this one. While many investigators of imitation concede to it a dominant position among the influences affecting the child's development, others would exclude it altogether. Now this disagreement could not arise from different conceptions of imitation, because the concept has been analyzed very thoroughly and in many different ways. American writings in particular are full of different classifications of imitation that bring out all its characteristic aspects. (A few of the investigators of this subject are: Lloyd Morgan, Thorndike, Berry, Watson, McDougall, Stern, and Guillaume.) To state the problem as I see it, imitation involves (1) configurations already belonging to an individual's equipment which are made to function by the performance of an act of the same kind on the part of another individual; (2) imitation may also be the arousal of a *new* configuration in an individual when he perceives some one else acting in a certain manner. Both types can be subdivided. Under the first type we can distinguish (*a*) instinctive, and (*b*) acquired configurations; and

under the second type we can define the level of the achievement demanded by the new configuration. To illustrate: (1*a*) A bird, seeing danger, emits a cry of warning, and this cry is repeated by other birds to whom the original cause is not perceptible. (1*b*) A familiar melody which one has heard may be involuntarily repeated. Performances at a low level of type 2 are those of repeating words never before heard —or, indeed, making any responses of an essentially motor nature. A performance at a higher level would be that of comprehending from a model how to solve a problem. In this connection we might think of the problems in Köhler's experiments. Extreme variations are possible in the perfection of these new achievements, because the new configuration aroused in imitation may be far less complete than its model; it may be far less precise; or it may even miss the point of the imitated action altogether.

It seems to me that this classification includes the important differentiation with respect to the nature of imitation which Lloyd Morgan has pointed out. We can perhaps characterize this difference by saying that imitation may either be of a movement or of a series of movements, or it may be of a purposive action. For a long time animal psychologists have looked only for the first of these types, and when they failed to find it, have concluded that there can be no general capacity for imitation. Thus, as an argument against imitation, Thorndike cites the case of a cat which pulled the loop with his paws, whereas the cat "(whom he saw) pulled the loop with his teeth." Berry, however, points out the fallacy of this argument, since it holds only for the imitative repetition of the movement.[355]

The distinction we have now made is, of course, subordinate to the main division previously drawn; because the higher the type of configuration arising through imitation, the more readily can the imitation be characterized with reference to the end sought. On the

other hand, the more superficial the configuration, the more the imitation is apt to be characterized as merely one of movement. The differentiation in configurations which here comes into consideration was discussed in the last section of the previous chapter (cf. pp. 242 f.). Just as drill-performances differ from intelligent performances, so the imitation of a highly significant action will differ from one of lesser significance. The more a performance is of a purely motor nature, and the more it depends upon inherited, or instinctive, configurations, the more it will appear to be a mere imitation of the movements being made. Yet even here, if one compares the movement imitated with its imitation, it is the integrated movement-melody of each which stands forth as being the common element. A photographic reproduction of the separate movements involved is never found. If an organism attempts to flee because it sees other organisms flee, what it imitates is the act of flight as a whole, and not the movements of the limbs themselves. In like manner, when I yawn quite involuntarily upon seeing some one else yawn (an example of the most primitive type of imitation), I open my mouth in my way, not in his way; for what I imitate is *yawning*, and not the movements of the other person's jaws. The less meaning an action has, the more difficult it is to imitate, even when the action is very simple. Guillaume reports of his child that at the age of nine months, he imitated brushing his own and his parent's hair, but twenty days later he failed to imitate the gesture of placing his hand on the top of his head. The difference which Lloyd Morgan makes out between the imitation of a movement and the imitation of a purpose is one of degree, and not of kind. As a matter of fact, there is no such thing as a pure imitation of movement. The difference is therefore one that figures only when we are dealing with questions of learning by imitation.

Hitherto, whenever one spoke of instinctive imitation,

one meant something else—namely, the connection between the perception which initiates the movement, and the movement carried out. Why does a bird repeat a cry of warning? This reaction has been considered an instinctive disposition by Lloyd Morgan and Stern; whereas other writers, such as Groos and Thorndike, refuse to accept this explanation, though they are unable to furnish a better one.[356] The problem of imitation for Thorndike is naturally quite unsolvable, because he has built his entire explanation of behaviour upon the theory of neurone-connections. A common imitative disposition, then, would require a web of neurone-connections so highly complicated that Thorndike is quite justified in rejecting it as altogether improbable. For similar reasons Groos also rejects an imitative disposition. Thorndike accepts, however, the instinctive release of a whole series of single movements, all of which would fall under our Group 1*a*.

Since we have rejected a theory of behaviour, including instinct, in terms of connections between neurones, to assume now an instinct of imitation as being a direct arousal of a movement by means of perceiving the same movement would be only a way of avoiding any explanation at all. I do not think we need be so sceptical. At the beginning of this chapter we became acquainted with a law of reproduction indicating that a configuration once given provides favourable conditions for the arousal of the same or of a similar configuration; and the behaviour of infants is altogether consistent with this law. Stern, following Baldwin, regards self-imitation as the first type of imitation to appear. A child will repeat the same reaction in endless monotony, whether it be a new manipulation or a vocal utterance. Since, according to Stern, this reaction starts as a purely motor mechanism, a connection is gradually formed between the movement and its perceptual result, so that in time the result itself will arouse the movement, thus giving rise to what Baldwin calls

a circular activity: R—P—R—P, where R is the movement of reaction and P the perception of this movement, or its result. On this hypothesis self-imitation is reduced to an associative connection. The fact that deaf children babble shows that utterances may occur altogether without the aid of hearing. Even in the case of normal children the first vocal utterances are not guided by hearing—a fact upon which Guillaume bases an argument against the usual explanation of speech in terms of auditory-motor associations. Yet the ear soon begins to play an important part in this behaviour, as indicated by the fact that the deaf babble less, and do not modulate their babbling like normal children.[357] It is highly improbable that the connection between hearing and utterance is acquired only by an external association. The dependency of our speech-apparatus upon hearing is actually much more direct. Many years ago Köhler called attention to the human capacity of singing a tone after it is heard; a capacity possessed to a remarkable degree by adults, and also by children at a very early age—having been observed even before the close of the first year of life. At the same time, Köhler sketched an explanatory hypothesis to cover the origin of this capacity. The following examples will indicate its early appearance among children. Preyer reports a little girl who could sing correctly a tone struck on the piano in her ninth month, and who, together with two of her sisters, sang before she could speak. Stumpf also tells of a little daughter of the well-known composer, Dvořák, who, at the age of one and a-half years, could sing melodies with piano-accompaniment quite correctly, even when they were rather difficult; and who in her first year began to repeat the march from "Fatinitza" after her nurse.[358]

That the relation of hearing to vocal utterance can not be a matter of mere associative connection, is also shown by other vocalic imitations. Long before they understand speech, children will imitate words more or

less clearly which they have not yet spoken spontaneously; and we have already called attention to the fact that vocalizations which occur spontaneously may be imitated at a very early age. (Cf. above p. 274.) Stern's daughter repeated *papa* at nine months for the first time, although this vocalization had not before appeared in her babbling monologue. In general, however, imitation at this age is practised more frequently with inarticulate noises: smackings, crunchings, and vocal cadences. In this connection we have an observation of Humphrey, reported by Preyer: "When about four months old, the child began a curious and amusing mimicry of conversation, in which she so closely imitated the ordinary cadences that persons in an adjacent room would mistake it for actual conversation." The articulation, vocalic organization, etc., was of course very incomplete.

The imitation of vocal cadences, or speech-melody, which Stern also observed in his daughter Eva at the age of thirteen months, has a special interest.[359] In some, though not in all, ways it is analogous to the repetition of true melodies; it certainly can not be explained in terms of associations previously acquired. At the time when speech is being learned, there begins a period of *echolalia* in which the child repeats with tireless continuation all the words or sentences it hears; either completely, or else their closing cadences.[360] This practice, and the direction it takes, are also very characteristic; for the child tries to make his imitations more and more like his models. In order to explain this behaviour, Claparède finds himself constrained to postulate an "instinct to conform";[361] but his explanation carries us no further than the assumption of an instinct to imitate. The facts recited have a general bearing, however, for parrots utter sentences chiefly with reference to their typical cadential characteristics, and one can also notice that they

practise until their imitations become more and more perfect.

If we could but assume an immediate structural connection between the perception and the movement, all these facts would be readily understood. The perceptual configuration would then reproduce the configuration of the movement, because of their inherent similarity; and the movement would then be a phenomenal copy of the perception, since the connection between these two distinct configurations (perception and movement)—which we have described as the reproduction of one by means of the other—must also involve a more or less definite and intimate structural connection. When the whole structure has attained a certain degree of firmness, this will be indicated by the stability of its separate members. The child then hears the spoken sound as something which is to be imitated, and he speaks in order to imitate the sound, hearing his own voice "as a more or less good replica of what he has heard." If the replica is "less good" it has the characteristic of incompleteness—of something lacking—an idea already familiar to us, and which of itself indicates that the performance is not yet over; that the organization can not stop here, since an end is attained only when the spoken sound has become a good copy of its model. In this way the configuration of a model and its imitation attains a state of equilibrium. There appears to be no need of a special instinct to explain this adaptation; because we are able to refer it, not only to the general laws of Psychology, but also to the laws of Physics.[362] Furthermore, this connection between perception and movement is of the same order as that which enables us to perceive the nature of the mental processes of others (cf. pp. 22 and 130 f.).

We have assumed the possibility of a configurative connection between perception and movement, and we have conceived this connection in terms of relatedness,

or similarity. It may, however, be of quite a different sort. When any one is solving a problem in my presence, I can imitate him if I understood the solution. In other words, if the perceptual configuration is properly aroused, so that, for instance, something previously indifferent now becomes the focus in which I apprehend the whole figure, the solution of the problem is immediately possible. There is nothing mysterious in the fact that a proper sequence of movements will follow upon such a perception. That is to say, we find here no special problem apart from the general one: how a voluntary act takes place at all—which we shall not now undertake to decide. I shall give an example of this type of response. In a game of forfeits, which children like to play, one child receives a spoon from another, and passes it on to his neighbour with the words, "Lirum-Larum Löffelstiel, wer das nicht kann, der kann nicht viel." The game is to receive and to pass the spoon along just as it was passed by the one who first held it; for instance, the spoon should be received with the left hand, and passed on with the right. Whoever makes a mistake must give a forfeit. It is interesting to observe how children, who do not yet know the game, learn it; the point being to find out what it is all about. This is the sole difficulty; as soon as the trick is comprehended, the problem is solved. The connection between the perceptual configuration and the movement to be made is therefore not itself a problem of imitation; because the imitation is essentially achieved as soon as the perceptual configuration arises in observing the other's behaviour. The problem with which we are here engaged does not involve our second form of imitation at its higher level; because the movement was already intended before the behaviour of the other person had been seen. A chimpanzee who attains fruit by imitation is already intent upon attaining it before the other animal has shown him how the act can be done. Similarly, when playing

a game of forfeits the child is from the start intent upon doing the right thing. But there are also instances of another sort in which the perception of the response calls forth an intention to carry it out; as, for instance, when a child of 0.11 dusts off a chair after seeing some one else do so. Such an imitation of "the 'serious play' type" also occurs among chimpanzees.[363] Yet even here we have nothing incomprehensible; because in grasping completely what has taken place, the connection of the result with the movement is at once apprehended, and consequently the perceptual configuration will itself lead to an imitation of the movement. One can explain this behaviour from different points of view. In the first place, the procedure has a certain similarity to the configurative supplementation spoken of at the beginning of this chapter; the movement is carried out because it is a part of the configuration. This would also explain why it is so difficult to pass by a piano, a letter-box, or a door-bell, without striking a key, opening the box, or pushing the button. Such a law, when applied to perceptual configurations, would constitute the factual content of a law formulated by James, Wundt, and others—the so-called ideo-motor law which Groos employs as the chief principle in explaining imitation, and which Thorndike, in turn, has so energetically opposed. The law is that every idea involves a tendency to movement, and is therefore itself capable of producing its appropriate response.

But one might also suppose that the impulse to carry out a copied action involves another law of configuration, which has to do with a tendency towards precision and fixity.

In reality this group of intended responses seems to stand between the two types of imitation previously discussed. In "intelligent" imitation, the model serves only to make possible the arousal of a correct perceptual configuration. The transformation into move-

ment requires no explanation, because the intention to perform the act was present independently of the model. In the first type of imitation, however, which may be illustrated by babbling, the model not only arouses the perception but also the impulse to imitate; and the transition from the perceptual to the responsive configuration is thus problematic. In the case mentioned of dusting a chair, the model furnishes both the perception and the impulse, and the transition from perception to movement requires an explanation no more than it did in the first type, because it belongs to the "understanding" of the act to be imitated. Thus, what is done is not directly an imitation of the movement made by another, but falls under the direction of the configuration perceived.

If a baby in its thirty-eighth week can strike two spoons together after seeing its mother do so,[364] we shall have to assume that the infant's comprehension of the mother's action is sufficient to call forth the appropriate movement; which is certainly no more of a riddle, and also no less of a riddle, than intelligent imitation.

With respect to the most primitive type of imitation we must therefore conceive the connection existing between the perceptual and motor aspects of the entire configuration as having a mutual intimacy. That we may and must accept this hypothesis for verbal and tonal imitation, has already been pointed out;[365] and there is no essential difference between these and other modes of behaviour. At first, imitation has to do with expressive movements. In the middle of the child's first year of life he can be made to smile by smiling at him, and likewise made to cry by appearing to cry before him. Even Thorndike recognizes an immediate and authentic imitation in the case of these expressive movements; including, indeed, a large number of others of the same sort, among which may be mentioned pursing the lips, which was

imitated by Preyer's son at the close of his fifteenth week. We have noted, however, in an earlier chapter that the emotions and their expressive movements must be very intimately connected (cf. above pp. 130 f.), and the transition from perception to movement must also rest upon this intimacy of the configurations involved.

Although we have not solved our problem, we believe we have indicated where the solution is to be sought, and how the connection takes place between lower and higher types of behaviour. The problem of imitation has thus been reduced to a very general problem of configuration—namely, how any perception can issue in movement. It is possible that the law of configurative supplementation, as well as the law of the repetition of figures, are both effective here. But there may also be other laws or peculiar emphases involved.

Since an intention to imitate can have various causes, we are led to the differentiation of two sets of problems: first, the necessity of imitating, and secondly, the ability to imitate. The whole problem of imitation has sometimes been considered as though imitation were compulsory; but that is a very one-sided view, for the essence of imitation is to be found in ability to imitate. It is no criterion of imitation to say that its intention arises entirely from perceiving the act of another. When a child learning to speak imitates everything he hears, this is certainly not a matter of "compulsory" imitation, because it is only *what* he says that is determined by the acts of another. Of course it is important to realize that a compulsion to imitate also exists, but this should be understood as something that arises from an ability to imitate. To be able to imitate means that a perceptual configuration may acquire a definite influence over a certain type of action; while the impulse to do anything at all may arise from some other source. Clearly, the more primitive an organism is, and the fewer factors there are to determine its acts, the greater must be the influence exerted by the con-

figuration of what is perceived. There must therefore be a gradual transition from an ability to imitate to the necessity of imitating; for at best the model imitated is only the strongest among a number of other factors which might determine an impulse to act.[366] Even in quite primitive cases of simple "susceptibility," this is true. When I am fresh I can quietly observe another person yawning without feeling obliged to yawn myself, but when I am tired his yawning not only determines *what* I shall do, but also *that* I shall do it. If I am very angry or very sorrowful I am less susceptible to another's laughter than I might otherwise be. It therefore seems to me that the problem of compulsory imitation is of less importance than the problem of ability to imitate.

Let us now pass from the general problem of imitation to the special one of learning by imitation. Here we have two possibilities. In the first, the individual learns by imitation to perform an already familiar act in a new situation. Imitation would then take the form 1*a* or 1*b*, and would occur without comprehension, being understood only after the movement is made. But imitation may also occur in accordance with the second form we have distinguished, in which case the imitation itself arises from and introduces a new configuration. It would appear from the facts as we know them that learning by imitation is essentially of this second type. At the lower stages babbling and verbal repetitions are clearly of this order. Again we may refer to Guillaume,[367] who maintains that progress in vocal imitation depends upon a preceding progress in auditory differentiation, that is, upon the establishment of new perceptual configurations. But even in other respects learning by imitation seems to be of this kind. The results which bear upon imitation in animal experiments are not altogether in agreement, yet this much at least may be said with assurance: that although imitation is infrequent, when it does occur it is highly

significant. Many of the negative results obtained in experiments on imitation are probably attributable to the fact that the investigator was looking in vain for an imitative performance at a very low level. Berry points out that his cats imitated only when they understood the act to be copied ; and Köhler writes as follows : " When any animal suddenly does manage to imitate a performance enacted before him of which he knew nothing before, he inspires the greatest respect immediately. Unfortunately this is a very rare occurrence even among chimpanzees, and when it does occur, the situation, as well as its solution, must lie just about within the bounds set for spontaneous solutions." [368] Köhler has reported several instances of imitation by chimpanzees ; and also one, equally significant, where a stupid ape, failing to understand the action of his comrades which was to serve him as model, committed ludicrous blunders. The latter instance is analogous to that of a man who, in relating a joke, misses the point.

Observations made upon children seem to agree with this view. Mrs. Moore expressly states that the boy whom she observed did not imitate to any extent until he had begun to understand other people's actions, and similar indications are given in Stern's observation that the continual repetition of speech only begins after an interest in, and a comprehension of, speech has already taken place.

Learning by imitation is, however, easier than spontaneous learning, and certain accomplishments like speaking and writing could not be learned at all without the aid of imitation. Somehow the situation is " improved " by the presence of a model ; both because the point of attack for the solution is emphasized, and also because the individual becomes more attentive to things not previously connected with the situation. Imitation becomes much easier when the action to be copied can be presented with the aid of language, so

that its essential features can be pointed out and clearly perceived. But it is no art to imitate movements that have already been learned or comprehended ; as Köhler remarks, chimpanzees also imitate without difficulty under these conditions.

In this sense, imitation is a powerful factor in development. Most of the things we learn are acquired, not through our own discovery, but through the comprehension of models or, in later years, by means of instructions expressed in language. Apprehension in this way constantly becomes an easier achievement ; though in the beginning imitation is scarcely less difficult than a new discovery. Despite these difficulties, man never learns so much in the same period of time as he does when a child ; and in childhood learning is always an achievement of a high order. Therefore one should not speak thoughtlessly of the actions of a "mere child ;" one ought rather to respect the period of childhood on account of the extraordinary fullness of its achievements.

§ 9—*Ideational Learning : Problems in Speaking and Thinking*

We turn now to the final aspect of learning, which we have called ideational. Perhaps the greatest number of problems, and certainly the most difficult ones, fall under this heading ; for it is through ideational learning that man frees himself from the perception of things present to his senses, and thus attains his mastery over the world. We shall consider here but a few problems that can be set within the framework we have already constructed. Many questions arise in this connection which, though they have been hotly debated, have not been satisfactorily explained by general psychology. We shall avoid these controversies as far as possible, referring the reader to the seventeenth chapter of Bühler's book for a statement of the points at issue.

Our consideration may be prefaced by the following

remark: The distinctions we have drawn, in order that we might trace the course of learning through individual performances, are so fluid that even when we are speaking of ideational learning we are always in the closest contact with learning of the perceptual and sensori-motor types. As we shall see, the most important categories appear first in perception.

We have now to consider the child's progress in learning to speak, for language is our most important material of thought. With language we can transcend the present; with its aid we can recall the past and anticipate the future. What do we know about this development which carries the human being so far? One fact, although not unknown, has received less attention than it deserves. I refer to the fact that a considerable degree of linguistic comprehension precedes ability to speak. Stern remarks that the difference between talking and comprehending what is said is never so great as during the first months of linguistic acquisition.[369] I myself have observed a little girl who could not speak a word, yet understood everything of importance that her mother said to her. After she had made it known that she desired something, her mother would ask: "Do you want some bread?" "Do you want some milk?" The child would then indicate her wants by a nod or a shake of the head. In this way a mode of understanding was readily established between the two. The mother also told me that this intercourse began early in the child's second year, and that the achievement of every new understanding gave the child visible pleasure. I may also add that I often employ a similar method with my dog.

There appears to come a time about the middle of the second year of life—subject to considerable individual variation—in which the child's vocal expression undergoes a sudden development. Previously, single words have been spoken as one-word sentences with a wish- or affective-character, although for several months

the child's primitive vocabulary may not have very noticeably increased.[370] 2 Now a sudden increase is noticeable in the number of words employed by the child, in connection with which the "name-question" appears as a typical phenomenon. The child points to all kinds of objects, asks "Wa's 'at?" and is satisfied when their names are told him. The name-question seems to be the more important factor, and the one upon which progress is chiefly based, for it may happen —as in the case of Stern's son—that an interval of some months will elapse between the appearance of these definite name-questions and the child's own use of the names given in reply.

The progress here taking place has been characterized by Clara and William Stern in the following way: The child now makes *the most important discovery of his life; which is that everything has a name.* Bühler also accepts this interpretation, and regards the performance as a "discovery," the real nature of which he then proceeds to analyze.[371]

3 Still a third peculiarity of this period may be mentioned, which differentiates it from the preceding period by conclusively showing that a transformation has occurred in the child's mode of relating the *word* to the *world*, or more particularly, the *word* to the *thing*. The general content of the one-word, and later of the multiple-word, sentence, which originally was but an expression of desire or emotion, is now itself altered. Along with these affective expressions, "material" determinations appear; that is, most of the child's talk is concerned with *naming* things. This is evidenced, first, by the fact that the use of interjections develops but little during this time, whereas the substantives, employed in these "objective" appellations, undergo rapid growth. The change is also manifest in the following displacement. Whereas substantives were previously employed as expressions of volition, interjections and words of demand are now used as substantive designa-

tions. "Hilda by degrees gets to use the petition *please* as the actual name of a roll, even when not asking for it. The exclamation *look*, often connected with the gesture of an outstretched first finger, comes at last to be used as the name for hands in that position ('nothing but looks,' said our daughter when she saw a picture of many such hands in an advertisement ").[372]

At this period of infantile development, the word frees itself from the wish-affective-relationship, and enters into a new relation with *things*. Things must have been present in the form of relatively firm and fixed configurations even before this time; for no one would or could assume that thing-configurations develop only in connection with their names. The behaviour of the child during his pre-lingual period would certainly not warrant such an assumption. Naming, of course, is not without its effect upon the thing-configuration. The problems of "thing" or substance will be considered in § 10.

The naming of objects is a discovery or invention of the child; and Bühler stresses the point that this is a perfect parallel to the inventions of chimpanzees.[373] Since we have already recognized invention as an act of configuration, it follows that naming can also be regarded as a configurative achievement; accordingly, we may infer that the word enters into the thing-pattern just as the stick enters into the animal's situation of "desire-to-get-fruit." It is then quite easy to assume that a word acts like any other member in incorporating itself into the pattern of a thing; that is to say, the name becomes an attribute of the thing—a possibility which Bühler has also considered. The name would then become a definite attribute of the thing, though it is not invariably present, because a thing may also be seen without hearing or seeing its name. So, too, the mother's eyes are a definite, though not invariable, attribute of her face, since they become invisible when her head is turned. This paradoxical constancy of a

variable attribute is common enough in our own experience. Thus, for instance, a blue dress will retain its blueness even when its colour can no longer be seen in the darkness. Yet the name is a peculiar kind of attribute, in that *anything* may possess it. Thus, a child can supplement anything with a name, and the name will then become the most pronounced character of the thing. In this way the ascription of a name will prepare the way for a further organization of the thing's attributes.

Even in our adult experience the fact that the name is an attribute of a thing, is not so strange as might at first be thought; for object and name do not always stand in a relationship so external as they do in cases where we call the mass m, the velocity v, etc. An anecdote will best explain what I mean by this. In a conversation on the value of different languages, Mr. Y. finally says: "The English language is the best, and I can prove it to you. Take the word *knife;* the French call it *couteau*, the Germans *messer*, the Danes *kniv*, while the English say knife, and that's what it really is."[374]

Certain facts from folk-psychology may also be cited in support of the hypothesis that a name is primarily the attribute of a thing. In primitive society the name given to a child is neither arbitrary, nor is it left to the fancy or discretion of the child's parents. Indeed, the name is not *given* at all; for since the child is only the reincarnation of a defunct ancestor, it already has a name when it comes into the world. But among many primitive peoples a man in the course of his lifetime acquires other and more important names than this. With each significant event of his life, such as the ceremonies at puberty, at marriage, at killing his first enemy, and at entrance into a secret society, he receives a name which is a mysterious symbol of the new "participation," of the new and mystical connections which have arisen within him. And what happens to real

names also happens more or less to all words, since in these primitive communities the distinction between individual and general terms is never so great as it is among civilized people. The speaking of words has magical effects, and for primitive people words occupy positions in the world-pattern quite similar to those of other objects and properties.[375]

This effect of words persists longest in fairy-tales, as Leo Frobenius has well shown in his tales of African tribesmen. The natives he has described differentiate sharply between *Tuschimuni* and *Mukanda* tales; that is, between their own legends, in which animals actually appear, live, speak, and act, and the tales of Europeans, in which one is merely told what took place "once upon a time." Sully also makes the following pertinent comment upon the influence of words in childhood: "This profound and lasting effect of words is nowhere more clearly seen than in the spell of the story. We grown-up people are wont to flatter ourselves that we read stories; the child, if he could know what we call reading, would laugh at it." In addition, Sully remarks "that to name a thing is in a sense to make it present." [376] Indeed, words have for children and for primitive peoples a quite different reality from that which they have for us. To them, words are not mere symbols, but have their roots in the world, upon which words exert a real influence (word-magic). It is therefore not so surprising that the youngest daughter of my friend and colleague, R. M. Ogden, should ask, at the age of 4.1: "Why can't we see what I talked?"

Our assumption that for children names are just as real as the objects to which they belong, finds ample confirmation in the ingenious investigations of Piaget. Although the children which he and his collaborators examined in various countries (Switzerland, France, Spain) were considerably older than those to whom we have previously referred, their experience was found to be of a radically realistic character up to the age of

seven years. "When a child learns the names of things he believes he has learned much more. He believes that he has penetrated into the essence of the thing and has discovered a real explanation of it." Originally names are actually *in the things themselves.* The things tell us their names; we need only to look at them in order to know their names. For instance, a child of four and one-half years on being asked how one knows that the sun is called sun, gave the following answer: "*I don't know—just because one sees it.*" Piaget writes that, to be exact, one must not say that the name 'sun' implies a yellow disc, and so on, but that the yellow disc, which is the sun, implies and contains the name 'sun.' In other words, names are in no wise arbitrary. Here are a few examples from Piaget's work. Question: Could one have called the Jura "Salève" and the Salève "Jura" (Salève and Jura being two mountain ranges near Geneva)? Answer by a child 7.0: "*No.*" New question: "Why not?" Answer: "*Because they are not the same.*" Another question: "Could one have given another name to the sun?" Answer by a child 9.0: "*No.*" "Why not?" "*Because the sun is just the sun, one could not have given it another name.*" However, another child (6.6), being put to it, admitted that God might have changed the names, although he insisted that in this case God would have done something wrong. Piaget follows the development of this nominal realism, and finds at a later stage when the names are no longer quite in the things, that they still "fit" the things.[377]

From a different starting point and with altogether different material—namely, sounds in the different languages of the world—v. Hornbostel has arrived at an almost identical conclusion. He maintains that "the meaning (sense, German "Sinn") determines the sound," and that "speech is sounded meaning." Originally the sound is completely adequate to the

sense and no experience, no learning, is required in order to understand what one hears." [378]

The naming-age of childhood raises many questions. If everything has a name, how does the child acquire all the different words he needs? This, indeed, is an important problem, which Stern calls the child's "need of words." [379] In satisfying this need, however, the child has recourse to other methods of finding names for things, beside that of asking questions.

1. Some names seem to occur as true inventions, about the origin of which we know nothing in detail. Mrs. Moore reports quite a number of such names. One of Stumpf's observations has perhaps a considerable significance in explaining how these names come about. His son gave the name *marage* to a building-stone of a peculiar shape, and he was able to remember it when he was seventeen years old, and give as the reason for his use of the word that "*the stone looked just the way the word sounded, and still does.*" In this instance we have an example of that original kind of inner connection between the object and its name to which both Piaget and v. Hornbostel refer.[380]

2. Words originally learned for a definite thing gradually extend their range of application. A thing whose name the child does not yet know may be given a name which is known to belong to some other object. As these transferences are of great theoretical interest, we shall give a few examples of them. One of "Hilda Stern's earliest words, employed even before the end of her first year, was *puppe*. Although used for the first time in connection with a real doll, the word was soon applied to her other playthings, like her cloth dog and stuffed rabbit; but for a silver bell, one of her chief playthings at this period, the word was never used." At the age of 1.7 this same child called the tips of her parents' shoes *noses*. "She then liked to pull our noses, and discovered the possibility of pulling likewise the

tips of our shoes." To a certain boy of 2.3, "*lala* first meant song or music; later, when he had heard a military band, it meant soldier, and finally all kinds of noises, including any unmusical sounds like claps and thuds."[381] Many such transfers have been described. They appear long before speech has arrived at the stage of naming (indicated in our first example), as is shown by Preyer's observation that his son uttered the word *atta* at the end of his eleventh month whenever anything disappeared—for instance, when a person left the room, or when the light was turned off. These transfers, however, maintain themselves during the naming-period, and again throw light upon the nature of naming itself. It does not appear that everything must have its own special name; if this were so, these transfers would not take place. It seems to be sufficient if a thing possesses any kind of appropriate name. Mrs. Moore reports, to be sure, that with the impulse to name things, both the number and the extent of these transferences decrease; but it seems to me that further observations are requisite before we can determine what influence the naming-tendency exerts upon the number and form of these transferences.

How can we understand the nature of these transfers? Bühler is right in comparing them with the transfers of chimpanzees; for instance, when the animal employs the rim of a hat as if it were a stick.[382] In no case dare we assume that the child confuses things for which he employs the same name. Mrs. Moore makes this point very clearly, by showing that behaviour towards different things bearing the same name may be quite distinct. So, for instance, her child called all little girls, Dorothy, but she showed signs of pleasure only in the presence of the particular Dorothy whom she knew, and from whom the name had been learned.[383]

Clara and William Stern write that at this age "the child's apprehension of its impressions is still so poor

and confused that differentiations are passed over which no adult could overlook." [384] But this is certainly an incomplete statement of the case. When a new thing receives an old name, we should interpret that fact by saying that the new thing enters into a configuration which was acquired with something else. The new thing does not need to be identical with this other thing, but only to possess certain characteristics which agree with the older configuration. What we must try to investigate is the configuration of each separate instance in which a thing and a name stand together.[385] Though we previously assumed that the name is added as an attribute of the thing, this is to be understood only as the general outline of an hypothesis to be filled in by further investigation ; for as Wertheimer has indicated, the characteristic configuration of a thing may greatly vary. " So, for instance, *red* in the statement that ' the wall is red ' is quite different from red in the statement ' blood is red.' " [386] These are problems for an investigation into infantile thought and speech to which, for lack of personal experience, I can only refer.

3. Finally, the child creates new names by combining old ones. Bühler recognizes the importance of this fact, and demands a systematic investigation. Pretty examples of this kind of naming were furnished by Stumpf's son, who until the fourth month of his fourth year employed only his own language—a language essentially made up of these combinations,[387] as may be indicated by the following samples :

hoto, horse ; *papn*, to eat ; *hoto-papn*, milk-wagon ;
loh, to run ; *hoto-loh*, mail-wagon ;
ei, egg ; *hopa*, to raise, to take up ; *ei hopa*, tea- or egg-spoon ;
wausch, meat ; *wausch-hopa*, fork ;
kap, broken in two ; *wausch-kap*, knife.

We adults would call this language something like a description, such as we might use when we do not know how to name a thing or event ; but in the

early stages of development these words have a true naming-function, inasmuch as the name is not yet a "mere" name. This primitive naming-function gradually undergoes a change, as is indicated when a child describes butterflies as "pansies flying"; [388] for here we seem to be at a much later stage of development where description and naming are no longer so much alike as they are at the beginning. The procedure, however, has not altered. The combinations which the child employs are also very instructive in understanding the configurations of thing and name. On the one hand, they indicate that the name does not connect itself with the thing in a purely external manner as the old theory of association would have us believe; for in that case one could find out the names of things only by questioning. Instead, the configuration tells how the thing should be called, so that one can see its name on it. On the other hand, it is interesting to see how the activities and the effects of things are employed in their naming. A thing is never isolated from its effects; for its effects belong essentially to its being. A fork, for instance, is not a metal object with four tines, but "something to eat with." Our conclusion regarding the effect of the thing is borne out by the investigations into children's definitions. The extensive observations of Binet upon his two daughters ($2\frac{1}{2}$–$3\frac{1}{4}$ years, and $4\frac{1}{2}$–5 years) are of especial value in this connection.[389] The younger child, as well as the older one, always answered questions of: What is that? (for example, a knife, roll, snail-shell, etc.) with a statement of purpose or action. This is also found to be true of children just entering school. To the child, therefore, a thing is not a completely isolated fragment, since the effective power and purpose of the thing adhere to it as a part of its essential nature.

Thus far we have considered the language used during this period of greatest speech-development only as a process of naming. Our conception of naming had

to be thereby adapted particularly to the facts involved. Yet this period of development should not be regarded too schematically; for though naming plays an important rôle at this time, language also serves other purposes. Even in addition to the retention of interjectional and volitional words, statements are also made by the child which transcend the naming of things, and indicate that even at this early age language may enter into relations with other configurations. The following is an example from Taine reported by Compayré.[390] A little girl eighteen months old greatly enjoyed the game of "hide-and-seek," in which her mother or nurse would hide behind the door and then call *cou-cou*. The same child had been told, *ça brûle* when her soup was too hot, when she came too near the fire, and when her hat was put on in the garden as a protection against the burning sun. One evening, on the terrace, when the sun was seen disappearing behind a hill, the child said: *a bule cou-cou*. Here was a process of uniting two event-configurations into one. The transference from a one-word to a many-word sentence is carried out, not, as Major reports in his observations, by making the child repeat two words which are somehow connected, but in a manner indicating a new and important achievement on the part of the child. At present, however, as Bühler remarks,[391] we do not know the exact psychological significance of such a performance. The problem is difficult, but its solution is nevertheless worth attempting.

Finally, Bühler has pointed out the following characteristic of the early language-period.[392] Very soon in the development of childish language there occur such general words as *this* and *one* (in the sense of "something"). Bühler observed these words when his child was 1.7, and they were not merely used in place of a definite name for something, but were correctly employed. "We had always the impression," he reports, "either that the more definite word did not occur to the

child at the proper time, or that, for unknown reasons, he was not concerned to find a more definite name." Bühler's interpretation seems reasonable if we make due allowance for his tendency towards *a priori* rationalism, and his dualism of mental contents and functions. Using our own terminology it may be restated as follows: To the child a thing is something which has a name belonging to its thing-character. Not only does the name *dolly* belong to the doll, and the name *mama* to the mother, but the naming-configuration is operative even before the name is given. The configuration demands supplementation, but this demand can be satisfied in other ways than by the particular name of the thing, so that under certain conditions a general word like *one* is quite sufficient. A word of this kind then becomes a sign for the completion of the general configuration of thing and name. In the previous case of the child's question: "What's that?" (cf. above) the word *that* does not perform this function, but merely indicates the incompleteness of the configuration; hence the *that* of the question is to be replaced by the name given in response. In the present case, however, *this* or *one* may take the place of a name for something which already has a name, and by this process of assigning names the thing-category is itself made clearer and more vivid.

Quite analogous is the use of the word *machen* (to do), which Bühler frequently observed as early as 1.5 in all kinds of combinations such as *snell machen*, *kaput machen*, *lala machen* (to sing), and quite generally in *so machen*. These are expressions for the relational configuration of happenings, just as *this*, *that*, or *one* are expressions for the configuration of a thing including its name.

As remarked at the beginning of this discussion, "naming" is but one among many means of approach to a study of linguistic development. Speech is not simply a matter of names and words. The specific

features of a given language are effective before the child is able to talk, even in the child's acquisition of names. As Guillet has shown, a two-year-old child learns foreign names of animals with much greater difficulty than those of his mother tongue.[393]

§ 10—*Categories of Thought: Substance and Causality*

We have been obliged to refer repeatedly to the fact that somehow or other the world of the child contains things and events. This fact involves problems which are both fundamental and difficult. At the end of § 6 we discussed the question why our world is made up of specific things, tables, chairs, post-offices, etc. Now we have to take another step backwards and discuss the substantiality of the phenomenal world itself. What does it mean to say that we are surrounded by "things"? It is impossible to separate this question from a similar one which relates to causality. What does it mean to say that one event causes another? These questions, of course, are asked from the point of view of genetic psychology, not from that of epistemology. However, these two points of view are so closely interconnected that any advance made in the first is bound to influence the second. If the question is asked how the first perception of a thing arises in the child, we may answer negatively that it would be wrong to suppose that the "thing" is nothing but a mere connection of various visual, gustatory, and auditory attributes resulting from frequent repetition; as, for instance, that the thing "mother" is a conjunction of the different "views of the mother" plus the impressions which the child gets from feeling its mother and from hearing its mother's voice.

Empiricism, our heritage from the philosophy of Hume, is untenable in the face of the facts. Unrelated sensations, which according to empiricism are the original data of mind, are altogether excluded from

our psychology. Moreover, as Piaget has pointed out, if empiricism were right, the child's phenomenal world ought to correspond more closely to the "real" world in primitive than in advanced stages of development. criticis In other words, if the organism were a mere passive recipient of influxes from the outside, then the impressions received ought to be in perfect harmony with the outside world; because the young infant would be unable to add anything from its past experience to what it receives. But this conclusion does not fit the facts. All through Piaget's investigation of many different fields, development takes an opposite course to that which the empiricist would lead us to expect. The original modes of perceiving the world, together with the primitive categories which a child formulates, are what we adults are inclined to call false representations of reality.

Again we must insist that in refusing to accept empiricism we are not obliged to embrace any sort of apriorism. In our previous discussion of this point (in the last section of Chapter III, p. 150) we based our argument against apriorism on the fact that very young children are able to understand quite adequately certain events, like the expressive movements of other people. This argument is inapplicable to the function of the categories, because these categories are far from being adequate in the beginning.

Even so, we need not accept apriorism; because apriorism has still another fatal difficulty. Let us assume that the organism possesses a number of against different structures, corresponding to substantiality, apriori causality, number, and so on, which it employs in order to "assimilate the action of the environment." Then we must ask the question: How does the organism discriminate in its use of these different forms? How does it know when to employ, say substantiality, when number, and so on? A second argument against apriorism, which has been used by Piaget, relies on

the fact that the categories show a progressive development. Were they original forms of the subject's mind, how could they ever be changed?

From this argument we can take a cue which will help us in building up a positive theory of the categories. For, if both empiricism and apriorism are wrong, we cannot accept a theory which is a mixture of the two, since such a theory would not be able to escape the objections which have been raised against empiricism on the one hand, and apriorism on the other.

What does the development of the categories signify? We can now refer to our discussion of learning in the last chapter where we have shown that learning is a re-configuration of previously established configurations. Under the pressure of a given situation, containing "external" and "internal" conditions, forms undergo modifications and alterations. But however far we go back, we shall nowhere find unformed "material." Nor can we distinguish between the material as "external" and the form as "internal." When applied to the categories this means that the world as it appears to the infant is already formed, and these forms are not added to unformed sensations by the infant's mind, but instead, are imposed on the child's mind by the situation in which the child finds itself. The term "imposed" is here intended to mean that under the conditions obtaining in each instance of sensory stimulation, the organism can respond only by a formed perception. Stimulation does not result primarily in an unformed "material" which must be changed by an added form of the mind. Instead, perceptual form is a direct function of both the outer and the inner conditions of the situation. In a previous discussion (pp. 120 f.) we have pointed out that a function of several variables is not necessarily the sum of the functions of each of these variables taken separately. Certainly it is not so in the case at hand.

The shape and the degree of cohesion of these per-

ceptual forms will depend on the state of the organism, and inasmuch as every new form-process changes the organism, new forms must constantly arise, which account for development. The data we already possess concerning this development prove conclusively that originally the degree of cohesion is very strong, while at the same time articulation is very weak. Progressive articulation tends here, as in many other cases, to decrease unity.

Let us now review some of the more important observations recorded by child-psychologists. Stern believes that he can ascertain a development which takes place in three stages. "The different points of view from which the world is mastered are not acquired simultaneously by the child, but they appear successively and in a cumulative fashion, so that what is old remains and becomes enriched by the new that is added to it. . . . The first stage of thinking is 'substantive': 1 from the chaos of unreflective experience the substantial is the first to work itself out into independently existing persons and things, as separate contents of thought. This stage is followed by a stage of 'action,' 2 in which the activities of persons and things are isolated in thought so as to attract special interest. But not until the third stage, that of 'relations' and 'properties,' 3 does the child develop a capacity to separate from the things themselves their inherent characteristics, and the varying relations which obtain among them."[394] According to Stern, these stages recur in each new kind of mental operation, so that a child may occupy simultaneously a high level with respect to an earlier accomplishment and a lower level with respect to a later one. Three such accomplishments which succeed one another, each having the same course of development, are: learning to speak, describing pictures, and remembering pictures.

The first point to be noted in this citation is the ambiguity of Stern's "chaos of unreflective experience."

If he means a chaos of unrelated sensations, he has made an assumption which we have already found reason to deny. It is also obvious that Stern's categories do not apply to "thinking" alone; for unquestionably they occur first of all in *perception*.

Aside from this point, however, there are certain objections to Stern's position, although his work is undoubtedly based upon a large number of observed facts. For instance, Bühler remarks that the sequence of categories in later achievements can not at once be likened to the sequence of earlier accomplishments.[395]

It must also be remarked that "properties" and "relations" belong with different configurations. A "property" or distinguishing feature is an evolution of the thing-pattern; as a thing emerges from its background it acquires internal articulation, without thereby losing anything of its unity or totality. A relation, on the contrary, generally obtains between several already isolated wholes, often quite distinct things. And hence, there arises a larger whole which includes these separate things, so that in a certain sense we may consider the relation as the internal articulation within this larger whole. But the question still remains: How closely are these two principles of configuration—property and relation—dependent upon each other?

We must also question, whether the substance-stage truly precedes all the others. "We know," writes Bühler, "that from the beginning the attention of the child fastens directly upon movements and changes. . . . Is the comprehension of activity actually retarded in the child's development?"[396] Guillet found that in looking at animal pictures a two-year-old boy was chiefly interested in the activity of the animals, less so in their forms, and least of all in their colours.[397] I might also add that although the child's first speech-sounds, exclusive of interjections, have a substantive character,[398] nevertheless, as Clara and William Stern themselves have pointed out: "The child's units of

speech do not belong to any particular word-group, because they are not separate words, but sentences." For this reason, Stern characterizes the first expressions of a child as " one-word sentences." " The *mama* of a child can not be translated into ordinary speech by the unit-word *mother*, but only by a complete sentence such as 'Mother come here,' 'Mother give me,' 'Mother put me on the chair,' 'Mother help me,' etc." Stern also observes that a modification in speech takes place when it becomes apparent to the child that each thing has a name. It is here that the substance-stage first makes its appearance, apparently in consequence of the *name-function*. Previously the state must have been different; for the active connection of objects and persons must have been given without any definite discrimination of things and their effects. The first configuration of order, then, is undoubtedly that of things; in this respect Stern's three stages are justified, with the provision, however, that they do not arise from " unreflective chaos," but from a kind of data which, though very primitive, are yet already formulated to the extent that both conscious things and their effects are contained within them (cf. p. 351).

One must not suppose, however, that a " thing " means exactly the same to a young child that it does to us. Certainly, right from the beginning " thing " means a definite kind of configuration in which the world appears to the child; a configuration whose connected membership is much firmer, much more intimately bound together, and the whole much more definitely particularized, than any mere set of external connections would allow. It is also a feature of the thing-concept that its configuration should have a core, or centre, to which the members of the configuration adhere in a definite manner; in other words, a thing has its attributes. It has often been remarked that nothing remains of a thing when its attributes are removed. But the inference that a thing is only the

sum of all its attributes would be just as false as the assertion that a forest is but the sum of all its trees. Just as in the case of the forest, the essential factor is the community of life; so, too, in the category of things it is a peculiar kind of cohesion which counts for most; and this can be described psychologically in no other way than by stating that these variable attributes adhere to a stable core. We are therefore led to assume that the thing is not constituted or constructed in the course of development out of previously existing attributes, but rather that the arousal of a thing-configuration means that a definite "figural core" enters into the child's phenomenal world. Certainly, the core is more original than the sum of its attributes, and since the organization of the thing takes place gradually, its separate attributes must appear very slowly.

However, in the beginning this thing-pattern is extremely dynamic. For us, the most static category is substantiality. For the child, it is very different. While we have separated substance as much as possible from force, to a child a thing and its effect can not be so sharply separated as they are in our thought. A mother, for example, is not only something which "looks so," and "is so," but more exactly something which "does this," "assists thus," or "punishes so." Nor does the effective side of a thing disappear when the child has attained the substance-stage of thought; for even causal connections which to us seem quite difficult, may be recognized early in the life of a child; although causality to a child is, of course, something quite different from what it is to us. For examples, a little girl of 1.11 remarked that "The wind make mamma's hair untidy; Baba (her own name) make mamma's hair tidy, so wind not blow adain (again)" (Sully); and a small boy of 2.7, holding his fingers before the sun, remarked that the "sun made his fingers bloody" (Scupin). We do not transcend the field of perception here any more than we do in the perceptual

configuration of an object. It would also be quite mistaken, I think, to say that in the first example the wind was perceived as one thing, and the untidy hair as another, the connection being added by the child-mind; or, in the second example, to say that the red appearance of the fingers and its connection with the sun was "thought out." From the point of view of a child these effects are described as simple facts of perception, just as if there were a single object with all its characteristics.

Piaget's last work shows that the original connectedness of substance and force is very close. Each substance possesses its own specific, intransmissible force which it does not lose while it causes its effects. Thus force is substantial and substance is dynamic at the same time.[399] Consequently only such things as are endowed with force appear to be substances. For instance, the child knows the air only as wind; hence, according to Piaget, he does not believe, even in the first half of his eighth year, that any air is in the room. Conversely, wherever there is an effect, there is also a thing. Thus 'cold' is considered to be a substance which possesses the force of producing wind.

It is obvious that with categories of substance and force so different from our own, the world, in practically every one of its aspects, will look otherwise to a child than it does to us. A child-psychologist will mark these differences in their various modes and developmental stages, as Piaget has so successfully done. Confining ourselves to the more general phases of the problem, we find that Piaget has distinguished seventeen different types of causal connection which occur during the process of mental development. Six of these are characteristic of the earliest stages—namely, motivation, finalism, phenomenal togetherness, participation, magic, and moral causality.[400]

Motivation, finalism, and moral causality are closely related, inasmuch as causal connection is in all these

cases a sort of psychological causation, with a double aim of teleology and utilitarianism; thus, the water of the river flows in order that it may come into the lake (teleology), and the clouds produce night in the places where men want to go to bed (utilitarianism).

Participation, as employed by Lévy-Bruhl—a very primitive connection, which tends to disappear in the sixth year—is a mutual action at a distance between things which appear to be in any way related. Thus, a draught within a room comes from the wind which is outside. Magic is a special aspect of this participation; it is a participation between the gestures, or even thoughts of the subject, and the things outside which make these gestures and thoughts influence the things. Phenomenal togetherness, as Piaget has shown, is on a different plane, because the child's categories are so highly dynamic that they can integrate into one sphere of being and happening things which have nothing in common except that they are contiguous in time and space. In other words, the connection between experienced events that belong together is so close—Piaget adopts Claparède's term, *syncretism*—that any partial event will serve as a cause or ground for any other. Thus, to the question, "Why doesn't the sun fall down?" Piaget received from a six-year-old boy the answer: "Because it's warm, it sticks." (*Parce qu'il fait chaud, il se tient*). Again, he put first a stone and then a piece of wood into a glass of water, each causing the water to rise. When he asked why it did so, a seven-and-a-half-year-old boy answered in the first instance: "Because the stone is heavy," and in the second instance: "Because the wood is light." Here we see an indifference towards conflicting statements, which, according to Lévy-Bruhl, is also a characteristic of primitive mentality.[401]

Thus Piaget's results are in harmony with Lévy-Bruhl's view of primitive mentality. And although there may be ground for the objection that Piaget's

results have been influenced, and his interpretations tinged, by his knowledge of Lévy-Bruhl, I still believe that he is essentially right. His work, however, ought to be repeated by other open-minded observers. In order to give an idea of the similarity between the views of Piaget and Lévy-Bruhl, I give Lévy-Bruhl's interpretation of the use made of "omens" in primitive society. To primitive people the omen of a flight of birds is at once a sign and a cause of an impending event, and the event can be altered by altering the sign, for instance, by turning one's body in such a way that the birds are seen flying from the left instead of from the right.[402] A similar connection appears in the familiar superstition of the new moon, which is a good omen when seen over the right shoulder, and a bad omen when seen over the left shoulder.

According to both Lévy-Bruhl and Piaget, primitive mentality knows nothing of *chance*, a conclusion which is in perfect harmony with our own view of mental development.

The characteristics of the child's categories of substance and causality reveal the general characteristics of his thought. In discussing phenomenal togetherness we have encountered an indifference towards conflicting statements. This is closely related to another feature of the child's mode of thought. In several experiments, which he performed on many children, Piaget found that children use juxtaposition in cases where adults use integration. Thus, in making judgments conjunctions are omitted, and in drawing pictures parts are placed side by side instead of in their proper positions. This characteristic of childhood seems at first to contradict the previously stated principle of *syncretism*; but Piaget points out that whereas *syncretism* means a firm cohesion of parts, the parts articulate very poorly with one another. Therefore when, for external reasons in thinking or drawing, a whole is broken up, it falls into unconnected fragments. The process of analysis,

so important to learning, appears to be an "all-or-none" affair with children. As for the process of unification, it is both difficult and rare. Piaget refers to this fact as the child's incapacity for synthesis.

§ 11—*Continuation : Number-Configurations*

We shall single out in conclusion one more category, in order to demonstrate how *different* are primitive configurations from those employed by adults; and how the attributes originally essential to a configuration may disappear, and be replaced by others entirely foreign to the original pattern. We refer here to *numbers*—the forms of thought which science has so highly developed. In a work of great significance for the psychology of categories, Max Wertheimer has investigated the kind of ideas employed by men who do not possess our developed number-system, in tasks where we should use numbers.[403] Wertheimer deals in the main with primitive peoples, but he gives some examples from infantile development, and he has also been able to demonstrate that even our "common-sense" numbers often differ in radical ways from the numbers of mathematics.

It is characteristic of our thinking that we are able to carry out thought-processes abstractly, with any sort of material, quite independently of the natural relations of things. This is not so in earlier stages of development, where the things themselves determine what kind of thought-processes shall be carried on with them. Here is an interesting example: "If one asks for the class-concept of two terms in this way, 'What are both *x* and *y*?' it often happens that among a number of 'right' reactions, a break suddenly occurs, and instead of classifying dog and cat as domestic animals, they will be called *enemies*." [404] In large measure this dependence upon the things themselves is found also in operations and constructions that are used in place of our numbers, and that we

may call "pre-numerical constructs." Our counting is transferred to whatever objects we may choose, and yet always remains the same; but order, natural modes of grouping, natural relationships of members and materials, remain more or less relevant to the pre-numerical constructs. Such a construct is the *pair*—as a pair of eyes; but a dish and a table are not a pair, nor are a stalk and a blossom. A pair is comprehended as being made up, not of "equal things, but of things that belong together"; man and wife, for example, are a pair, called a married couple. The same holds true for a group of three members: two adjacent trees and a third one farther off are not necessarily a natural group of three. Bühler cites a pretty example from the investigations of Decroly and Degand,[405] where a child of 4.9, who had learned to comprehend a group of four members, was asked how many cherries there were when a pair was hung over each ear. The child always replied: "Here's a pair and there's a pair." The Sterns [406] also cite the case of two children of the ages 2.7 and 2.10, investigated by Major and Lindner, who were able to understand and make use of "two apples," but not of two eyes, two ears, etc. This observation may appear singular, because paired members form so natural a group to us adults; and Decroly and Degand found that the child they studied knew and understood the two-ness of eyes, legs, stockings, and gloves at the age of 2.2. The paradox is explained, however, when we consider that pre-numerical constructs do not have the same characteristics that our numbers have. With numbers, two is always the same, but with "two eyes, two boards, and . . . two fighters, each pair gives rise to a different configuration of two." [407] Consequently, when the child has learned the two-ness of apples it does not follow that he is now able to transfer this configuration to pairs of quite a different constitution. In this connection Wertheimer observed that for children

three single nuts have the designations of one, two, three; while three objects which lie in a certain order have another designation; namely, that of the form in which they are arranged. Thus, children are often greatly astonished to find that the five-spot domino corresponds to the quantity five. We ourselves reckon apples by the bushel and years by the score.

To how slight an extent numerical constructs are transferable is shown by the following observation of Friedrich, reported by the Sterns. A child 4.3½ was asked by his grandfather: "How many fingers have I?" to which the child replied: "I don't know; I can only count my own fingers." This is not an instance of mere mental incapacity, but is largely a result of natural factors which oppose the transfer of an operation from one material to another. When a pot is broken in two, it is unnatural to say that two things have been made out of one; the natural thing is to say that the pot is in shards or fragments.

Long before the first number-words are properly employed, pre-numerical constructs play a part. The following experiments can be made in the first months of the second year: when a child plays with two or three identical objects, such as beans or coins, no distinction is made between them; but if one of them is taken away, while the child's attention is diverted, its absence will be noticed immediately, even when the order of those remaining is also changed. On the other hand the removal of one from a large number of objects will not be noticed. As Wertheimer maintains, the formation of natural groups and conglomerate constructs is genetically prior to counting.

Counting is a supplemental process, occurring first as a serial arrangement about the beginning of the second year. Apples, blocks, buttons—always things of the same class—are arranged in series and the child says *one, another one, still another one*, etc., or *button, another button, still another button*; but never simply

apple, apple, apple.[408] When we recall what was said about the first use of the word *one*, it will be clear that serial words, such as "another one," etc., stand for things seen and employed as members of a series already present or in course of construction. This implies a step in the direction of counting and the conception of numbers. But serial construction and group-construction remain different processes, even after the child has learned to count properly. The Sterns give the following example of their daughter, Hilda, aged 3.7. When five fingers were held out before her, and the question asked how many fingers there were, she would count them correctly. If, however, the question was asked over again, How many fingers are there? she would begin to recount them each time the question was asked. The last finger was indeed the fifth, but the total number of fingers did not yet mean to her the sum of five.[409] Thus, as Wertheimer has pointed out, many peoples use other number-words in counting than the ones they use in naming sums.[410]

The pre-numerical constructs of children are, of course, constantly influenced by association with adults; and hence they do not become stable enough to show their capacity in performance as they do with primitive people. Wertheimer says of the constructs employed by primitive people, that they are both less and more effective than our logical constructs: "Less, in that certain operations of thought commonly employed by us are altogether excluded from consideration; more, in that the thinking itself is in principle more intimately concerned with real things." The release from reality, which is both possible and easy to our mode of thinking, is a specific product of our civilization. The child must go a tremendously long way in a short time in order to learn to think like adults, in a manner which is not at all natural to him. To lead him along this way, so that his advancement may be vital to him; this is the difficult though grateful task of the teacher.

CHAPTER VI

THE WORLD OF A CHILD

IN this brief concluding chapter I shall try to indicate some of the important features of the child's world, as contrasted with the world familiar to us adults. The child's sphere of interest has been called a world of *play*, a world of irresponsibility, in which unreality reigns supreme. This characterization merits a closer scrutiny.

The problem is not identical with that of understanding children's play; for real play, in its very beginnings at least, occurs so early in life that we can not yet speak of a conception of the world, even in the simplest literal sense of the word. On the other hand, there appears in later types of play only one aspect of what we really have in mind; for the distinction we adults draw between play and serious endeavour is certainly a quite different matter to the child. Even if the child does not really play, still his world has some of the characteristics of play. In other words, certain peculiarities in the play of children, and likewise in the play of adults, are also found in the internal and external behaviour of children when they are not playing. We must not forget that the child grows up in a world controlled by adults, and is constantly subject to their influence. We have to deal, therefore, not with a set of conditions that remain unaltered for a long period of time, but rather with a view of the world, which view is constantly undergoing a process of transformation—sometimes more rapidly, at other times more slowly. This fact must be borne in mind

when we try to set forth the characteristics of the child's world.[411]

I choose the following example as a starting-point for our discussion. A child may play with a stick of wood and treat it as a "dear baby." Yet a short time later, after being diverted from play, the child will break the same stick of wood, or throw it into the fire, without the slightest compunction.[412] How can these two different types of behaviour toward one and the same object be reconciled? Superficially considered they seem to be altogether incompatible; for the first action is carried on not less seriously or intently than the second, which makes it impossible to suppose that when acting as if the stick of wood were a living being the child is only playing, whereas in destroying the stick he has taken into consideration the real character of his plaything. In many ways it is apparent that the matter can not be so simple as this. One can see that a child manifests a deep and genuine feeling for his playthings; for intense emotions can be provoked by interrupting the play-situation. Sully has given numerous examples of this fact.[413] "One little boy of three-and-a-half years who was fond of playing at the useful business of coal-heaving would carry his coal-heaver's dream through the whole day, and on the particular day devoted to this calling would not only refuse to be addressed by any less worthy name, but ask in his prayer to be made a good coal-heaver (instead of the usual 'good boy'). On other days this child lived the life of a robin redbreast, a soldier, and so forth, and bitterly resented his mother's occasional confusion of his personalities."

We must conclude from this statement that the child takes his play very earnestly; how earnestly, can be seen from the following observation of Frobenius: "A professor is working at his desk, while his four-year-old daughter is running about the room. Her commotion disturbs him; so he gives her three burnt matches,

and says: 'There, go and play with these!' The child sits on the floor and plays with the three burnt matches, which she names Hansel, Gretel, and the Witch. All goes well for a time; when suddenly the child startles her father with a frightened shriek. 'What is the matter? Has anything happened to you?' he asks. The child runs to her father with evident signs of fear, and says: 'Father, father take the witch away. I am afraid to touch her.'"[414]

It is apparent from such an instance as this that we cannot define play as the antithesis to earnestness and work; we must try to find some other distinction.

It is characteristic of much of the play of children (most typically in their play with dolls) that inanimate objects are treated like living human beings. This observation is generally confirmed, and applies to their behaviour in a much wider sense than we have attributed to play. "That is to say, the child sees what we regard as lifeless and soulless as alive and conscious."[415] Sully gives some pretty examples: the little boy twenty months old who had a special preference for the letter W and always used to speak of it as "dear old boy W"; and the youngster of four who drew an F by mirror-writing, and then, putting the correct letter to the left of it, F ꟻ, cried out: "They're talking together."

Miss Ingelow remembered that when she was two or three years old she "used to feel how dull it must be for the pebbles in the causeway to be obliged to lie still and only see what was round about. When I walked out with a little basket for putting flowers in I used sometimes to pick up a pebble or two and carry them on to have a change; then at the farthest point of the walk turn them out, not doubting that they must be pleased to have a new view." It seems to me quite incorrect to speak of a propensity for *personification* in this case, meaning that children first have perceptions like our own, and afterwards endow them with life by inference from the analogy of their own experience.

A similar view, however, has long been held in folk-psychology; and the theory of animism advanced by English investigators, and seemingly confirmed by an immense amount of material, rests upon this basis. The universal animation which primitive peoples find in nature has thus been taken to be an explanation based upon rational inferences from human behaviour to the behaviour of things. To-day this theory is assailed on many sides, and in what follows I shall cite some of the chief objections to it advanced by Lévy-Bruhl in his important work on the subject. I may refer the reader, also, to the excellent and easily accessible little book by K. Th. Preuss.[416]

Animism cannot be conceived as an "explanation" of the world, for, in the first place, the life of primitive man is such that he could not be expected to take any interest in theoretical explanations. In the second place, primitive man needs no explanation; for the disconnected things which the philosophy of man has slowly exposed to view, do not exist for him at all. The theoretical exponent of animism seeks to give a plausible explanation of the facts of folk-psychology by imagining how he would himself come upon such ideas if he were at the same level of civilization, and placed in the same surroundings, as primitive man. But in so doing the theorist makes the mistake of identifying primitive man with himself; an error like that committed by a well-known biologist who, having succeeded with the aid of a microscope in perceiving a retinal image in an insect's eye, concluded that what he saw was what the insect must have been able to see.[417] To a psychologist the error is as clear as day. What one sees through the microscope are only the objective factors which may be effective in the insect's vision; but what the insect actually *sees* when it has this definite retinal image, it is quite impossible to observe. The same is true regarding the theory of animism. The environment of primitive people, and their peripheral

sense-organs, are approximately known to us, but we can no more draw conclusions regarding their minds from this knowledge than we can in the case of the insect. Our perception is a product of development, and, as we have seen, the child perceives things differently from the way we perceive. The development of perception depends upon the total environment, upon the milieu, and above all, the sociological conditions of this milieu. Lévy-Bruhl attaches a special importance to the last point, and for this reason. Man grows up as a member of society—and with primitive people internal social connections are much stronger than with us—so that man's entire development, including, of course, his perceptions, is dependent upon society. To give a rather weak analogy, we have found that language—which is a *collective* factor—plays a very important rôle in the first development of perception.

There is really nothing to explain; for "primitive people do not perceive as we do." Our "natural things" do not enter into their perception at all. Before the characteristics can arise which are all-important to us, the primitive mind finds other and *mystical* characteristics in things, by means of which they are perceptually connected. The connection itself, therefore, is not a problem, but something given; and the question arises how development has been carried on so as to loosen these originally fixed connections.

Since everything that exists for a people at this stage of civilization possesses mystical qualities which are much more important to them than "natural" characteristics, our distinction between the living and the dead, the animate and the inanimate, can have no meaning to them. Rivers, clouds, winds, even the main directions of space, to mention but a few examples, all have their mystical powers. The distinction between animate and inanimate is a product of development; at the beginning no such question could possibly arise, since everything, including even directions, names, and

words in general, possesses its active principle as an immanent attribute.

When we return now to consider the child, it is evident that this view can be directly carried over to him. We should not suppose it " natural " that a child would first be able to see dead things which he later endows with life ; for the original situation is rather one in which the characteristic of effective activity belongs to everything alike. The discussion of the thing-category in the preceding chapter has already led us to this conclusion, and Bühler also reaches it when he says : " The child knows absolutely nothing of life and mind, but is acquainted only with purposive events." Unlike the poet, a child is unable to breathe life into a dead thing.[418] He must gradually learn to make the distinctions we make, and these become an acquisition of his perceptual categories.

If we ask what are the criteria employed in deciding whether a thing has life or not, we can answer only by investigating the behaviour involved ; for the answer depends upon the kind of place a thing occupies in a larger course of events, or in a more comprehensive dynamic structure. Consequently, when the child learns to distinguish between the animate and the inanimate, this more extensive structure must still be involved. Here the child's progress is very slow, since, according to Piaget, causality to a child under seven or eight years is much the same as psychological motivation, to which Piaget applies the term *pre-causality*.[419]

There is a similar process in the development of such categories as " mere appearance," which we can already trace to some extent in certain cases of children's activities. After the child has learned to reach for an object seen—an achievement discussed in the last chapter (pp. 270 f.)—this object possesses certain visual and tactual characteristics belonging to its configuration as a thing. The child is, therefore, constantly grasping at spots of light, shadows, and the like, and must

gradually learn that there are things which may be seen, but which can not be grasped or touched. The behaviour of children in response to reflections in a mirror is of especial interest. Preyer describes his son's development in this respect very thoroughly. At first the image was not seen at all, later it was smiled at and grasped at, then it was reached for as if it lay *behind* the mirror, and finally movements of avoidance were observed; for the child looked away when the mirror was held up before him. At this stage of development, the reflection apparently frightened him, as being something which did not fit into any of his patterns. Dogs probably never get beyond this stage. I have observed this behaviour in my own dog, and up to this point it coincided perfectly with Preyer's observations. The first time the dog came before a large mirror standing on the ground he ran up to his image barking loudly, and with a great show of excitement. Later he ran to one side of the mirror and stuck his head between the mirror and the wall. Since that time he has taken no notice of reflections in the mirror, and, like Preyer's son, he turns away his head whenever I hold him in front of a mirror. But development proceeds rapidly with a child. In two weeks' time after the sixtieth week of life all shyness before the mirror had been overcome by Preyer's son, and some preparation, at least, had been made for a correct understanding. Yet the child still grasped for and, indeed, struck at, his image. But soon this behaviour also ceased and the child employed the mirror thereafter just as we do.[420] Cases of an opposite sort have also been observed, in which a child demands to see something invisible that has been felt; as, for instance, in the case reported by Sully of a little girl not yet two years old who wanted to see the wind.

The original thing-phenomenon, in which the visual and tactual are closely related, must therefore be broken up in certain instances, and new patterns formed in

which there may be only visual, or only tactual, constituents. Something similar must take place in drawing the distinction between animate and inanimate; except that this process is much more complicated and difficult, since the configurations involved are themselves much more extensive. It is not to be wondered at if this process goes on for a long time before it is complete enough to afford a clear-cut differentiation. And, even after the basis for this distinction has been roughly laid, that will not prevent the old undifferentiated pattern from again and again reappearing. I would even go so far as to say that vestiges of these old patterns frequently recur in the everyday life of adults; and not merely in the form of superstitions, either.

If it be asked what kind of a pattern gradually enables us to distinguish the dead from the living, one might frame an answer in terms of the original "expression" of perceptual phenomena. Although, at start, all phenomena are expressive, they are not all expressive in the same degree—a fact which we have found to be influential in the development of perception (cf. pp. 311 f.). It is possible that the distinction between the living and the dead arises from this difference in degree of expressiveness; for certainly a pencil has very little of this quality, whereas a snake, even though stuffed, has very much of it. In other words, along with the progressive evolution of perceptual configurations goes the differentiation of their expressive qualities, so that the distinction of the living from the dead would proceed directly from the development of single percepts.

In addition, I might venture to assume that this distinction is also gradually drawn from the consequences of the child's behaviour with the things in question; for gradually, though very gradually, of course, the child will notice that things react in very different ways. On the one hand, he will meet with resistance from "living" things; he must approach

them and shape his reactions to them quite differently than with inert things. On the other hand, the learning of this distinction may be made more difficult by the fact that "living" things conform more than inert things to the wishes of the child; thus displaying, in a certain sense, an opposing behaviour. Two examples from Sully illustrate this point: A little girl of five one day stopped her rolling hoop and exclaimed: "Ma, I do think this hoop must be alive, it is so sensible: it goes where I want it to." In another example this pattern of connection was "falsely" employed; or, as we should say, cause and effect were interchanged. A little girl scarcely two years old said to her mother during a rainstorm: "Mamma, dy (dry) Babba's hands, so not rain any more."[421] The distinction may also involve emotions, since one can bring pain to the animate, but not to the inanimate. Thus the child notices that his little brothers and sisters react to ill-treatment quite differently from his doll. All the same, it seems to me a tenable hypothesis that the distinction is facilitated by these consequences; and that the child must learn to consider his behaviour with respect to its consequences, and in this way come to look upon his conduct as the *beginning* of a series of interrelated events.

We see now why development must proceed so slowly. A child's ability to bring the present into relation with the past and the future is quite inadequate, as Stern has pointed out.[422] Even after a beginning has been made, the total connections of the world and of life are by no means grasped at once; for smaller and more limited relationships must first be built up which, as we shall soon see, can exist in relative independence of one another. Here, again, the mentality of the child is related to that of primitive man; for to the primitive mind time is so vague a concept that in many societies no anxiety at all is felt for the future.[423]

Returning again to the problem from which we

started out—namely, the question as to the nature of childish play—it seems to me that we can best understand play, psychologically, by considering the activities of the child from the point of view of the larger configuration of events in which play occurs. We have, at the beginning, a situation in which the child can form no temporal patterns at all that go beyond the activities being performed. Here all separate act-complexes must be independent of one another, every one being of the same sort and of the same worth. From the child's point of view, there is as yet nothing at this stage that can be called play or not-play. From an adult's point of view, however, childish behaviour at this stage can be characterized as *playful*, provided one accepts Groos' definition of play as an activity enjoyed purely for its own sake.

Gradually the child perfects his temporal patterns, and it becomes a characteristic of these that many of them can exist side by side without very strongly influencing each other. I believe that two systems arise: one is concerned with undertakings, processes, and things which relate somehow to adults, while the other is independent of adults. Thus to a child the world of adults separates itself slowly, and at first indistinctly and obscurely, from his own child-world. "There is nothing to show," writes Piaget, "that a child holds the same objects to be real when he is alone, and when he is in the company of adults." [424] The difference between these two worlds is obvious in the formal nature of the child's language. As M. E. Smith has shown, conversation with adults tends to lengthen a child's sentences.[425] The world of the adult makes itself gradually felt by the child through the unpleasant consequences of certain acts. In the adult's world the child is not free, but meets with compulsion and opposition which are lacking in his own world. So long as the connection between the child's world and the adult's world remains a loose one, motives for

drawing new distinctions, such as that between the quick and the dead, are doubtless found to be stronger in the adult's world than they are in the child's where no such requirement is made. If a child finds himself in his own world, these categorical analyses are largely lacking from both his external and internal behaviour; therefore he reacts in the same way towards both animate and inanimate things.

But we must proceed still further. The relative independence of different patterns obtains not only for the two great groups constituting the child's world and the adult's world, but also for individual connections within each of these worlds. While the adult's world, by virtue of the principle that distinguishes it from the child's world, soon forces itself to be comprehended as a *totality*—so that the independence of individual actions, one from another, gradually disappears—it is quite different in the other world where, to-day, the child may be a coal-heaver and to-morrow a soldier. Here a stick of wood that has just been cuddled may the next minute be thrown into the fire. Yet in the child's world these different actions do not interfere with one another, because they have no more connection with one another than they would have for us in our play. The jack of diamonds may be a tremendously important card—" the right bower"—when I am playing euchre, but is a relatively unimportant card when I am playing bridge. With us adults, of course, there is always a conformity to the " rules of the game," which are fixed and valid in each kind of game; whereas a child's play is not bound by extraneously determined rules. Yet the lack of connection between different games is the same in both cases. The hard and fast connections that pervade our world are but a result of the domination of our non-play life; whereas to a child this domination is not originally present, and only gradually introduces itself.

Finally, the illusion indicated by the child who plays

with a stick of wood as if it were a doll, can also be explained in terms of our principle. As a general rule, the "illusion" will be no greater when the plaything is more nearly true to nature. Favourite dolls are not necessarily the costliest products of the toy-shop, they may be the simplest, rudest, and more or less damaged specimens. This would be very remarkable if we were to identify the child's world with that of the adult, and regard each separate thing in the position it occupies in the general and all-inclusive relationships of life. Yet adults have reasoned that because a doll is so very different from a living child, one should therefore make it just as like a child as possible. Accordingly, we have dolls fitted out with mechanisms that close the eyes when the doll is laid down, and which produce vocal sounds when it is squeezed. We have beautiful dolls, with genuine or deceptively similar hair, and with clothes correct in every detail. To a child, however, a doll is never a part of the adult-world—or, at any rate, it becomes so only after it has been taken away from the child for protection or as a punishment—and since the doll does not occupy a fixed position among definitely regulated things, the entire assumption upon which dolls are usually made is a false one. It is sufficient for the child if something is there to satisfy a present want; and the thing, whatever it be, will then have all the characteristics necessary for this purpose. Since a stick of wood can be caressed, it becomes at once a baby that can be loved and cuddled. The fact that it does not have certain other characteristics belonging to a real baby does not come into consideration at all; because the need of harmony with the rest of experience simply is not felt. To a child there is as yet no *single* world-all in which each particular object has its manifold relations.

Ethnological analogies to this interpretation are also available. For example, primitive peoples do not know

anything about a single world-all which binds everything together. What is right for the white man may be altogether wrong for them. If a white man shoots an invulnerable fetish-bird, this does not destroy the bird's invulnerability in their eyes, but only shows that different charms work for the white man. This primitive view of the world lacks the criterion by which we endow with reality only such things and processes as are perceptible to *everyone*. Furthermore, those things which are visible only to the chosen few—the medicine-men—are held to possess a peculiarly magnified and important reality.

Finally, as we have already pointed out, and can now confirm by examples, the characteristics that are important in the world of primitive peoples are entirely different from those that are important in our own world. This difference is apparent in primitive drawings and their relation to reality. Here we enter a sphere which, externally at least, is very similar in nature to the child's world we have been describing. Spencer and Gillen report the following observation in Central Australia. The natives maintain that certain drawings are made only in play and have absolutely no meaning; yet *precisely the same* sketches have a very definite significance when they are attached to objects found on consecrated ground. The explanation of this phenomenon, to us wonderful and mysterious, lies in the fact that while our basis of judging the relationship between an image and its object is their likeness, for primitive people the basis is a common participation of image and object in the same mystical power. Consequently it is quite impossible for us to interpret these drawings. A similar conclusion was reached by Volkelt from experiments with children. Placing an object and two drawings of this object before a group of children, he asked which of the two drawings they considered the better representation. By combining in pairs all kinds of drawings, varying from the most

primitive child's drawing to a reproduction correct in perspective, he was able to determine which representation seemed the best for each individual child. Volkelt's general conclusion was that the similarities, equalities, and differences between two figures are essentially unlike for the child and for the adult.

Parkinson reports from the South Sea Islands that a drawing which had been taken for snakes was really intended to represent a pig; and that another figure, which might perhaps have passed for a face, was really a club. The natives were greatly astonished that any one should ask the meaning of these drawings; for they could not imagine how any one could fail to understand the drawings at once.[426]

It is not unusual, therefore, that a thing should first derive its being and significance from the relationship in which it exists. So long as no larger connection binds everything together, a stick of wood may quite well be a trifling thing at one moment, and a dear dolly the next.

To say, then, that a child experiences no genuine illusion in his play means, according to our view, that the object under consideration is perceived in an illusory manner only so long as the child is in his own world. But he may at any time slip out of his world into the world of the adult, and then the object will be treated differently. Yet when a child is absorbed in play, no part of this other pattern of the adult-world is necessarily present. Groos accepts this view,[427] and so does Piaget, who writes: "Play is the only reality in which the child believes; therefore reality itself is something a child likes to play with adults." But while the play of children is an autonomous reality, we must remember that the "true" reality to which it is opposed is much less "true" to a child than it is to an adult.[428]

In general, my view of the child's world finds support and a significant supplementation in the studies of Piaget, who reports that the demand for verification

is a consequence of association with others, and therefore a sociologically conditioned phenomenon. For this reason, verification belongs to the adult's world before it belongs to the child's world. Piaget distinguishes four stages in the childish consciousness of reality: (1) whatever corresponds to desire is real (up to the second or third year); (2) there develop two or more different but equally real worlds (up to the seventh or eighth year); (3) there begins a hierarchical arrangement of these worlds (up to the eleventh or twelfth year); and (4) this hierarchy is then completed with the aid of logical thought.[429]

What is most characteristic of the child is his own child-world, which to him is more important and dearer than the world in which the adult dwells.

The child's world is in a high degree *egocentric*. Piaget, who has traced this characteristic through many different types of behaviour, thinks that egocentricity is perhaps the fundamental feature that distinguishes the minds of children and of adults.

However, although the child's world is egocentric, this does not imply that the child discriminates in the same way as the adult between the Ego and the Non-Ego. According to Piaget, who takes a very extreme view, the consciousness of the Ego is not primitive at all. Instead, all the early experiences of the child float in an undifferentiated "absolute." The Ego is not a primary datum but a result of dissociation. Piaget believes that the infant even feels pain, like a prick on the sole of the foot, without referring it to his Ego, the pain floating in a space accessible to everybody. Admitting that the Ego—World distinction undergoes general development from very inarticulate beginnings, I am not yet convinced that this "absolute" of which Piaget speaks is ever quite undifferentiated as regards the Ego. The mere fact that the Ego is the dominating centre of the child's world ought to give this centre a specific character.[430]

For a long period of time the child makes no less progress in his own world than he does when he comes under the influence of the adult-world.[431] Furthermore, when the distinction between these different worlds begins to be known, when the child himself begins to speak of playing, this play-world becomes all the more vivid to him. A stimulating anecdote reported by Sully illustrates this fact. " One day two sisters said to one another: 'Let us play being sisters.' "[432] Since the sister-configuration comes from the adult-world, or at any rate belongs to it at that stage of development, what was here proposed was to take this pattern over into the child-world in order to give it a quickening reality.

The instance of Stumpf's son, who spoke his own language for a whole year, and could not be broken of the habit by any admonition, is a good example of the fact that occasionally even performances which bring the child into relation with adults, and which are of special importance in the construction of the adult's world, may originally be carried over entirely into the sphere of the child's world. Stumpf writes of his son: " When we corrected him and said: 'This is called snow,' or 'This is called milk,' he would still answer: '*Ich kjob*,' '*Ich prullich*' " (these being his expressions for the same things).

We have already mentioned the sudden transition to normal speech, which Stumpf explains as follows: " The psychological motive was very simple; the child had grown tired of the game. He may also have felt, at last, that the deviation of his language from the vernacular, and its incompleteness, were both disturbing and humiliating "—which no doubt was true. The adult's world had become so powerful that it was now the child's ambition to attain it, rather than to make use any longer of the possibility of remaining in his own world, which had now assumed a somewhat contemptible character. Thus, after a time, the child became ashamed of his own language.

Another modification in the boy's speech appears to me to have been a harbinger of this transformation. For a long time the boy called his brother Rudi, *Olol*, and himself *Job*. But there came a time, even while he yet spoke his own language with reference to everything else, when he avoided these names and would only say, and give heed to, the names used by adults. "*Job weg, Liki da*" (Liki being the name which adults called him—an abbreviation of Felix); "*Olol Job ä—Rudi Liki haja*," meaning that Olol and Job are bad names, while Rudi and Liki are nice.[433] An encroachment of the world of the adult upon the child's world is manifest in this instance; which Stumpf very pertinently describes by saying that the old name *Job* no longer seemed *worthy* to the child. Slowly the child's world must give way to the world of the adult, and the case of Stumpf's son gives us a good view of this process.

There is, however, one sphere of interest which children learn from adults that has a very close and intimate relation with the child's world; and that is the sphere of religion. To a child, religion is something tremendously serious; perhaps, indeed, truly "holy"; and yet despite, or better still, because of, this, it is completely incorporated into the child-world. As the adult sees it, the child *plays* with religious things. The Christ-child and the Christmas-eve manger with its figures of men, angels, and beasts—these are realities corresponding to the child's world; for they are things to which the customary laws of the adult-world no more apply than they do to any other playthings.

Mrs. Else Roloff has carefully observed and reported upon the religious play of her two little girls.[434] The plays, which centred about the Christmas festival, were of peculiar significance. "Before the festival, Eva was 'the Christ-child.' She flew through the room with outstretched arms bringing gifts to all the children. . . . The little one also claimed an office, and was promoted to the position of the 'Angel Choir,' to sing and mingle

with the angels. After Christmas eve the holy persons were represented by building blocks. . . . In Heaven—a stage-like structure, with stairs leading up to it—stood the Saviour, the Lord Jesus, the 'Guest,' and the Christ-child. The relation between the Christ-child, the Saviour, Jesus, and the Guest, was remarkable. The children knew quite well that the Christ-child was called Jesus, and that He grew up to be the Saviour, yet they maintained that *their* Christ-child was always small. They knew, too, that at our meals we 'pray that Jesus may be our *Guest*'; but this did not hinder them at all from attributing a separate personality in their play to each of these names."

This last observation is of especial interest; for it illustrates a peculiarity of childish thought that may extend far beyond the province of religion—a peculiarity so clearly brought out in this instance only because the material was especially well adapted for it. This peculiarity is related to a characteristic which has already provided us with a key to so many features of the child's behaviour—namely, the relative independence of different configurations. Jesus, the Saviour, the Christ-child, and the Guest can be united in the structure of a single personality, although it is doubtful whether a configuration possessing all these separate properties (for the names are properties) could ever be experienced simultaneously by a child. Nevertheless, the smaller constellations of the main configuration may be entirely self-sufficient, and exist side by side without disturbing one another; indeed, they may exist in the same form in which the total configuration exists. To us, such a procedure would mean a logical contradiction, but, as Mrs. Roloff has shown, there is no such contradiction in the child's mind. The author quite properly refers to analogies from folk-psychology; for neither does this contradiction of thought exist at primitive levels of civilization. Here a thing can have two forms at the same time, and can also occupy

different places. That it is the *same* thing despite these, to us incompatible, circumstances, arises from the fact that the total configuration in which the identity is contained is quite different from the one we know. The development of our whole- and part-configurations is based upon the principle of non-contradiction; but in the case of other civilizations and, as we now see, in the case of our own children as well, the process is different; the principle of contradiction being irrelevant because such things as liveliness, active power, and mystical characteristics are of far greater importance. Thus the fact that three objects, which were once but the names and attributes of a single personality, should nevertheless become three different and independent beings is not at all disturbing to the primitive mind.

If we trace this development still further, it appears that the configuration of the adult-world constantly assumes a greater extension, so that the complete independence of different worlds is no longer possible. The school acts as a very important agent in this process; for in school there are both work and play. What was once a world of equal rank with the adult's world gradually becomes a mere matter of play. Before this takes place, however, the real world occasionally forces itself into the child-world, so that even in his play the child is now and then conscious of the fact that there is another world beside his play-world—one in which his play is not to be taken seriously. At this period the stimulus to play may even be heightened by the fact that the play-world is devoid of all responsibilities. Groos describes this behaviour as follows: "Think, for instance, of the laughter of romping boys, which serves to reassure the combatants by its implication that, in spite of appearances to the contrary, the fight is only playful." [435]

Yet play always remains a sphere relatively shut off from the rest of the world, and for a long time illusion

—as we have employed this term—retains its power. What a child will do only with the greatest reluctance when he thinks he is working, he will do with burning zeal in play—see Mark Twain on Tom Sawyer's stratagem for getting the fence whitewashed.

But the opposite may also occur ; for play itself can be so directed that it becomes closely related to the rest of life. The play-character then almost entirely disappears. One may take part in a game of chance simply because one is in a mood to play, and then suddenly notice that in the course of the play one has become so deeply involved that to lose the game would be a very disagreeable experience. Thenceforth the play-mood is all gone, and the outcome of the game becomes a serious decision of fate.

The foregoing example is chosen, not from the life of a child, but from the experience of an adult ; yet I believe that the chief feature of an adult's play is that it still belongs to a " world-apart," and that whenever we play we step out of the usual relationships of life—thus professional gamesters can not be said to " play."

This development, as we have here sketched it, is characteristic of *our children ;* but play is to be found in every other type of civilization, and likewise among animals. Our theory, however, can not at once be carried over to these other manifestations of play, since the distinction of the two worlds, which to us is so marked, either does not exist at all, or is a quite different affair in the lives of animals and primitive men. An investigation into the psychology of these other kinds of play is no part of our task. Nevertheless, we shall complete our sketch with a few words upon the biological significance of play, in order to bring it more closely into connection with certain things that have been discussed.

We noticed that the child acquires many, and not the least significant, of his accomplishments from his child-world. When he lives in this world he does

what we objectively designate as *play*, in accordance with the definition already given (cf. p. 375). Now Groos has advanced the opinion that the play of children is of tremendous biological value in preparing the child for serious endeavour. "I find this value," he says, "in the *indirect* benefit, both physical and mental, which must be ascribed to play by way of practice and preparation."[436] In the second chapter (p. 43) it was stated that childhood is the time for learning; and that the longer the period of infancy, the more the individual can learn. The theory of Groos is in perfect harmony with this idea. If play is of service to animals, then, according to Groos, one should not say that animals play because they are young and joyous; but rather that animals enjoy a period of youth in order that they may play. In both of his excellent books on play Groos sustains his views with such a mass of material that his theory is now universally acknowledged.

Yet I must warn the reader against an over-estimation of this theory. Not only should one guard against a false pedagogical application of play which smuggles artificial and foreign aims of instruction into the child's world (to this Bühler has already referred in the conclusion of his larger work), but one should also remain unprejudiced by theories of play, as applied both to children and animals. Instead, these intensive expressions of vitality ought to be taken into account as they are, without considering any aim whatsoever. Play is but one type of behaviour among others. While a relationship, of course, exists between all kinds of behaviour, any procedure that brings all behaviour under the single head of practical utility is distinctly one-sided; it has led to many errors, sponsored by the rationalistic utilitarianism of the last fifty years. Much of what was once called a "biological explanation" is no longer so regarded. Since we now know that biological development does not take place by chance-

variation and the selection of what is useful, working hypotheses based upon these concepts are no longer explanatory, or even in a true sense biological.

The question has also been asked what are the effective causes which, in any particular instance, lead an individual to play. An explanation based on teleology is no real explanation, but at best indicates the direction in which an explanation may be sought. The child knows nothing of the end to be fulfilled by his play. Of the many theories on play, the most famous is the Schiller-Spencer theory of "surplus-energy." In addition, the "recreation" theory of Lazarus has its merits. The main points of these theories can easily be gathered from the names that have been attached to them, and an exhaustive discussion of the subject will be found in the works of Groos.[437]

Bühler contributes a new suggestion in pointing to the fact that, aside from any consequences whatsoever, all activity brings *pleasure*. I should modify this statement by adding that a *successful* activity—one that brings something I desire, or that achieves what it should—brings me pleasure, whether the end attained be itself pleasant or not. We have already met with examples of this fact; I may recall Köhler's experiment with the double-stick which Sultan fitted together, and continued to employ, even after he had brought all the fruit within reach. Bühler regards this "functional-pleasure" as the motor which drives a disinterested activity of play.[438] I find here a very suggestive idea, but one that has yet to be developed into a theory; for it is certainly no simple matter to comprehend theoretically the transition from pleasure to action. There nevertheless can be no doubt that the pleasure taken in an achievement operates as an incentive to new achievements.

It is not my intention to classify the forms of children's play; classifications are found in the works

of Groos, Bühler, and Stern. Accordingly, our discussion of the subject ends at this point.

In this book I have tried to give an introduction to the study of child-psychology by pointing out the principles in accordance with which the behaviour and development of the child may be comprehended. But the reader must not conclude from my book that I think all the riddles have been solved, or all questions answered. The general aim of my book has been but to point out a way in which the solution of these numerous problems can be attained. The nature of mental development as it has been revealed to us is not the bringing together of separate elements, but the arousal and perfection of more and more complicated configurations, in which both the phenomena of consciousness and the functions of the organism go hand in hand.

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NOTES TO CHAPTER I

(1) This statement holds also for the principle of convergence advanced by Stern, and frequently employed by him in child-psychology (cf. p. 53). The principle is derived from more general considerations, as may be seen from Stern's philosophical works (cf. especially *MP*, pp. 95 f.).

(2) I have adopted the terminology employed in my essay on Psychology (cf. *Lehrbuch der Philosophie*, ed. by Max Dessoir, Berlin, 1925, *2*, p. 502). McDougall gives a threefold division similar to the one that follows (cf. *OP*, pp. 3 f.). Our discussion agrees also with the "personalistic" position taken by William Stern in child-psychology (cf. *PC*, pp. 52 f.).

(3) I have recently discussed this problem from the point of view adopted in this book. Cf. "Zur Theorie der Erlebniswahrnehmung," *Annalen der Philos.*, *3*, pp. 375-399. "Introspection and the Method of Psychology," *British Journal of Psychology*, 1924, *15*, pp. 149 f.; also my essay, cited above.

(4) This conclusion can not here be more definitely established, but, in my opinion, it excludes the possibility, not only of mensuration, but also, contrary to appearances, of any true enumeration of the phenomena. On the other hand, certain applications of the concept of function seem to furnish Physics with an equivalent for quality.

(5) We can altogether disregard the problem how we know anything of the consciousness of our fellow-creatures. Our previous criterion was simply the possibility of communication. We shall return to this subject later in the text (pp. 22 and 130 f.).

(6) Compare the following also with G. Kafka's discussion, pp. 6 f.

(7) Rubin reports the converse proposition, that one can follow the contours of a figure without making eye-movements, as, for instance, on an after-image. In this case the impression is always given that eye-movements are actually being made. Cf. Rubin's book referred to in note *133* on p. 399.

(8) We must decline to enter here into a criticism of psycho-vitalism. The argument of the text is directed less against this theory than it is against many other current modes of explanation in psychology. In my opinion, the difficulties with which a psychological theory of consciousness has previously been burdened are now overcome, so that one of the main stays of psycho-vitalism has fallen away.

(9) Thorndike, whose position is in many ways close to that of the behaviourists, also employs the term behaviour, as we do, so as to include the phenomenal aspect of conduct.

(10) Cf. my *Erlebniswahrnehmung* (note *3*).

(11) Cf. W. Köhler, *PhG*, pp. 192 f.; also "Die Methoden der psychologischen Forschung an Affen," *Handbuch der biol. Arbeitsmethoden*. Edited by Abderhalden, Abt. vi. Teil D, pp. 69 f. And

compare "Bemerkungen zum Leib-Seele-Problem," *Deutsche Mediz. Wochenschrift*, 1925, No. 28.

(12) Fundamentally their physiological theory is only a translation into physiological terms of the psychological atomism which they have rejected, as I have pointed out in my review of Watson's Psychology (cf. *Psychologische Forschung*, 1922, *2*, pp. 382 f.). No physiological theory can be independent of psychological theory. This does not imply an explanation in the sense attacked above, but simply an adequate treatment of the facts. Thus, the analysis of consciousness into sensations would be a psychological theory even if one proceeded to explain the isolated sensations physiologically; and likewise it would be a psychological theory if one were to deny the concept of sensation, and substitute another, for which a physiological explanation would then have to be sought. Cf. also the references in note *3*, and the detailed criticism of the entire movement by A. A. Roback (*Behaviourism and Psychology*, 1923). Instructive, too, is the sharp discussion between Watson and McDougall in *Psyche*, 1924, *5*.

(13) Köhler, *I*, p. 63 (87).

(14) *Ibid.*, pp. 64 f. (89).

(15) Cf. E. C. Tolman, "A Behaviouristic Theory of Ideas," *Psychol. Review*, 1926, *33*, pp. 353 f.

(16) Cf. Köhler's discussion of consciousness in animal psychology, *OU*, p. 56 note, and his remarks concerning the observation of conduct and consciousness in the *Pedagogical Seminary*, 1925, *32*, pp. 681 f. Köhler's article in Abderhalden's *Handbuch* (note *11*) states the problem of our knowledge of other selves, and suggests a solution. Scheler, also, in his book on *Sympathy* treats of the perception of another person's mind, his views being in some respects the same as our own. (Cf. M. Scheler, *Wesen und Formen der Sympathie: Die Phänomenologie der Sympathiegefühle*, 2nd ed., Bonn, 1923, and Buijtendijk, F. J. J., and Plessner, H., Die Deutung des Mimischen Ausdrucks, *Philos. Anzeiger*, *1*, 1925, pp. 72 f.) Among the younger behaviourists, cf. E. C. Tolman, "A New Formula for Behaviourism," *Psychological Review*, 1922, *29*, pp. 44 f., reviewed by me in *Psychologische Forschung*, 1923, *3*, p. 409. Thorndike has given excellent descriptions of the conduct of monkeys; but despite his evident talent for observations of this kind, he attaches small weight to them, as his theoretical conclusions will testify. Cf. *AI*, pp. 193 f. (p. 18 of the original publication, "The Mental Life of Monkeys," *Psychol. Review Monograph Series*, 1901, 3). The difference between the scientific attitudes of Thorndike and the present writer have been pointed out by Norma V. Scheidemann in the *Psychol. Review*, 1926, *33*, pp. 64 f.

(17) *Z*, p. 58.

(18) To be sure, now one part and now the other, is more strongly developed according to the living conditions of the animal. Cf. Edinger, *Z*, p. 59.

(19) *Z*, p. 507.

(20) Under certain conditions experimenter and observer may be the same person.

(21) Cf. with this, Chap. IV., p. 247.

(22) Cf. Bühler's discussion, *GE*, pp. 56 f.

(23) Révész, G., "Über spontane und systematische Selbstbeobachtung bei Kindern," *Zeitschrift f. Angewandte Psychol.*, 1922, *21*, pp. 333 f.

(24) In Chap. IV, p. 211, an application of this procedure to animal psychology is described.

(25) Detailed instructions for planning and keeping child-diaries are given by Stern, *PC*, pp. 42 f.

(26) Cf. the report of H. Volkelt, "Fortschritte der experimentellen Kinderpsychologie," in *Bericht über d. IX Kongress f. Exper. Psychol. in München*, Jena, 1926, pp. 80 f., and A. Gesell, *The Mental Growth of the Pre-School Child*, New York, 1925.

(27) Cf. the book by O. Lipmann and H. Bogen, *Naive Physik: Theoretische und Experimentelle Untersuchungen über die Fähigkeit zu intelligentem Handeln*, Leipzig, 1923, in which the writings on this subject are completely surveyed.

(28) O. Külpe, "Psychologie und Medizin." *Ztschr. f. Pathopsychologie*, 1912, *1.*, p. 12 of the separate edition.

NOTES TO CHAPTER II

(29) Cf. Stern, *PC*, pp. 51 f.

(30) Cases are not here considered in which the conditions of life of an individual or of a species suddenly undergo a marked change.

(31) Cf. in this connection, Lloyd Morgan, pp. 18 f.

(32) *PS*, pp. 299–300.

(33) Bühler has recently spoken of the "chimpanzee-age" of the child. *GE*, p. 81.

(34) *I*, p. 66 (91). Cf. also his description of the animal's behaviour when touched with an electrically charged wire, *I*, p. 58 (81).

(35) R. A. Acher, "Spontaneous Constructions and Primitive Activities of Children Analogous to those of Primitive Man," *Amer. Journal of Psychology*, 1910, *21*.

(36) A clear and straightforward presentation may be found in the *Naturphilosophie* of E. Becher, *Kultur der Gegenwart*, Leipzig and Berlin, 1914.

(37) Claparède, p. 188.

(38) *Sp*, p. 263.

(39) Cf. Groos, *SK*, p. 8.

(40) *PC*, p. 298. In other places Stern advances other views which I can not list here. He finds the inner essence of human unity in recapitulation, and speaks of the common heritage of the entelechy-character which passes from the species to the individual. But these conceptions can be understood only in relation to his philosophical system, and are consequently outside the range of our discussion. Cf. *PS*, pp. 324 f., *MP*, p. 110.

(41) Cf. Claparède, pp. 188 f.

(42) *PC*, pp. 51 f. Cf. also *MP*, pp. 95 f.

(43) Most of the psychological text-books—and especially the large works of Ebbinghaus, Wundt, and Watson—contain detailed descriptions; as does also Becher's *GS*.

(44) Edinger, *Z*, p. 461.

(45) *Z*, p. 522. Some time ago I discussed Edinger's views in a brief paper entitled: "Ein neuer Versuch eines objectiven Systems der Psychologie," *Ztschr. f. Psychol.*, 1912, *61*.

(46) *Z*, p. 523.

(47) L. Edinger and B. Fischer, "Ein Mensch ohne Grosshirn," *Archiv. f. d. ges. Physiol.*, 1913, *152*, pp. 26–27.

NOTES TO CHAPTER III

(48) Soltmann, "Über einige physiologische Eigentümlichkeiten der Muskeln und Nerven der Neugeborenen," *Jahrbuch für Kinderheilkunde*, 1878, *12*.

(49) Cf. M. Gildemeister, "Über einige Analogien zwischen der Wirkung optischer und elektrischer Reize," *Ztschr. f. Sinnesphysiol.*, 1914, *48*; also P. Cermak and K. Koffka, "Untersuchungen über Bewegungs- und Verschmelzungsphänomene," *Psychol. Forschung*, 1921, *1*, especially pp. 100 f. The term *fusion* which is used in the text corresponds with customary terminology, but tells us absolutely nothing in regard to the theory of the phenomenon. A theoretical discussion will be found in the second article cited above.

(50) This number is dependent upon so many factors that we shall here be obliged to content ourselves with an approximate statement.

(51) With very rapidly moving motion-pictures a different phenomenon appears. Motion is again lost, and one sees the moving object multiplied. For example, a gymnast jumping over a horse may be seen, during the jump, with six stationary legs. The same phenomenon of multiplication is well known under a light operating on alternating current—as produced, for example, when the hand with outspread fingers is moved rapidly back and forth.

(52) Cf. Preyer, I, p. 44, Bühler, *GE*, p. 103, Moore, p. 57, and P. Guillaume, "Le problème de la perception de l'espace et la psychologie de l'enfant," *Journal de Psychologie*, 1924, *21*, pp. 122 f.

(53) It is not denied that experience may be involved in the development of seen-movement, but the question is *how?* Cf. my article: "Über den Einfluss der Erfahrung auf die Wahrnehmung." *Die Naturwissenschaften*, 1919, *7*, pp. 597 f., and my minor study in *Psychologische Forschung*, 1922, *2*, pp. 148 f.

(54) The authors do not draw this conclusion, but are very cautious in expressing themselves on this point. Cf. *op. cit.*, p. 1.

(55) *Ibid.*, p. 4. Also, when Preyer asserts (I, p. 214) that a child born without a cortex produced crude sounds when his back was rubbed, this does not seem to have been an altogether normal reaction.

(56) Preyer, I, pp. 196–7.

(57) *PC*, pp. 68 f.

(58) As Preyer points out, such movements may, of course, under certain conditions be directly harmful. Thus a child may during sleep open one eye with a movement of its hand, and go on sleeping with this eye open.

(59) Readers desiring to familiarize themselves with the extraordinarily interesting but by no means simple physiology of the reflexes should study C. S. Sherrington's book, *The Integrative Action of the Nervous System*, New York, 1906.

(60) The same state of affairs is naturally to be found in the sensory field, where it is referred to as the "law of specific sense-energies" (Johannes Müller). Also, the processes which take place in the sensory centres of the brain as correlates of the pheno-

mena of sense-perception are the specific processes characteristic of distinct domains. The reader will find a brief presentation of the data on this subject in an article by W. Nagel, "Die Lehre von den spezifischen Sinnesenergien," *Handbuch der Physiologie*, edited by W. Nagel, 1905, *3*, p. 1.

(61) For an orientation into the complicated subject of eye-movements, which can only be touched upon here—and also for the facts concerning space-perception in general—the following book is recommended: St. Witasek, *Psychologie der Raumwahrnehmung des Auges*, Heidelberg, 1910. References to other original sources will be found in subsequent notes.

(62) Ewald Hering, *Die Lehre vom binokularen Sehen* (first part). Leipzig, 1868, pp. 3 and 22.

(63) H. v. Helmholtz, *Handbuch der physiologischen Optik.*, 3rd ed., revised by Gullstrand, von Kries, and Nagel, *3*, Leipzig, 1910, p. 48.

(64) Cf. Stern, *PC*, pp. 69 and 77 f.; also Guillaume, *op. cit.*, p. 122.

(65) Cf. with this, Hering, *op. cit.*, pp. 18 f.

(66) *Ibid.*, pp. 22–23. Köhler has observed the same relations in the co-ordinations of chimpanzees, *I*, pp. 173 (239).

(67) Cf. von Kries' statement in Helmholtz's book cited above, p. 514 (note).

(68) *Ibid.*, pp. 511 f.

(69) Cf. *PB*, pp. 264 f.

(70) Cf. *PC*, p. 78. This behaviour was observed occasionally, though not regularly, even as early as the fourth day after birth; likewise in Scupin's son.

(71) *Op. cit.*, p. 122.

(72) Cf. *GE*, p. 103.

(73) Cf. Bühler, *GE*, pp. 102 f.

(74) Bühler, *GE*, pp. 103 f. The italics are mine. Bühler reaches no decision whether the connection is inherited or acquired, and whether it is brought about by maturation or by experience.

(75) This last behaviour is designated as the *principle of the greatest horopter*. Cf. with this, E. Hering, *Beiträge zur Physiologie*, *4*, Leipzig, 1864, pp. 261 f.

(76) Hering's principle of avoiding illusory movement, *ibid.*, p. 265 f. Helmholtz's related principle of easiest orientation (*op. cit.*, p. 55), which Hering attacks, shows by its name the close relation between seeing and eye-movements.

(77) *PhG*.

(78) Cf. Köhler, *PhG*, pp. 27, 201–2, 262–3; also a fuller discussion in his article: "Gestaltprobleme und Anfänge einer Gestalttheorie," *Jahresbericht über die gesamte Physiologie*, 1922, pp. 512 f. The citation in the text will be found on p. 536. Investigations which A. Marina published, first in 1905, and then in revised form in 1910, are in full agreement with Köhler's point of view. Marina operated upon apes, first by exchanging the medial rectus and lateral rectus muscles of an eye, and later so as to substitute the superior rectus for the lateral rectus. In the first case, therefore, the eye was moved outward by the previously inward-moving muscle, and *vice versa*. In the second case the muscle moving the eye outward was eliminated and its place taken by a lifting muscle. If a definite impulse were conducted from the centre through the

pathway to each muscle, the animal must have made the most remarkable eye-movements after the wound had healed. But, instead, the voluntary and automatic sideward movements of the eyes were carried out in a normal manner as soon as the cicatrization was complete. From this and other results the author concludes "that the anatomical association-pathways from the centres to the eye-muscles are not fixed," and that the *conduction pathways have no predetermined function*. Considering the results of other operations of transplantation, he seeks to justify his attribution of a very general significance to this conclusion, and demands a new foundation for the physiology of the brain. The validity of his inference and its bearing upon brain-anatomy and physiological psychology is also admitted by Ziehen in a review of one of Marina's investigations, though out of respect for the older theory Ziehen thinks that certain errors have probably been made in the experiments. Cf. A. Marina, "Die Relationen des Palaeencephalons (Edinger) sind nicht fix," *Neurol. Centralbl.*, 1915, *34*, pp. 338-345, and the review of this article by Ziehen in the *Zeitschr. f. Psychol.*, 1915, *73*, pp. 142–3.

(79) Cf. Guillaume, *op. cit.*, pp. 125 f., and his Monograph: *L'Imitation chez l'Enfant*, Paris, 1925, pp. 46 and 60. Preyer, I, p. 79; Shinn, I, pp. 22, 109 f., and 129. Hetzer, H., and Tudor-Hart, B.: "Die frühesten Reaktionen auf die menschliche Stimme," *Quellen und Studien zur Jugendkunde*, her. von Charlotte Bühler, Heft 5, Jena, 1927, pp. 107–124; and B. Löwenfeld: "Systematisches Studium der Reaktionen der Säuglinge auf Klänge und Geräusche," *Zts. für Psychol.*, 1927, *104*, pp. 62–95. Concerning the theory of the auditory direction of perception, cf. E. M. von Hornbostel: "Beobachtungen über ein- und zweiohriges Hören," *Psychologische Forschung*, 1923, *4*, pp. 64 f., where a complete bibliography will also be found.

(80) Cf. Thorndike, *EP*, p. 48; Preyer, I, p. 256; Watson, *PB*, p. 262.

(81) Preyer, I, p. 259; Watson, *PB*, pp. 259 f.

(82) According to Compayré, I, pp. 83–4.

(83) Cf., above all, Lloyd Morgan, then Preyer, I, pp. 236 f., and James II, pp. 383 f.

(84) Morgan, pp. 122–4. A similar observation may be found in James, II, p. 400.

(85) James, II, p. 385. James' chapter on instinct is written with all the charm of his vivid style of presentation. Although I can not subscribe to his theoretical conclusions, I recommend the reading of this chapter most highly. A brief statement of the history of the concept of instinct can be found in Groos, *PA*, pp. 25 f.

(86) Cf. *B*, p. 106. In *PB*, Watson allows this definition to stand as a "mere schematic outline" (p. 255).

(87) Thorndike, *EP*, p. 1.

(88) Cf. E. C. Tolman, *loc. cit.*, p. 355.

(89) *EP*, pp. 123 f.

(90) *EP*, p. 134, repeated from *AI*, p. 35.

(91) Cf. Hans Driesch: *Philosophie des Organischen*, 2nd ed., Leipzig, 1921, p. 321, and *GS*, pp. 397 f. Driesch holds that no definite proof has yet been given for the existence of "specific, individualized, instinctive stimuli."

(92) Cf. Köhler, *I*, for example p. 46 (63), and elsewhere.

(93) In a recent paper H. C. Link has reached conclusions similar to those appearing in the sequel of our text. Cf. "Instinct and Value," *American Journal of Psychology*, 1922, *33*, pp. 1 f.

(94) L. L. Thurstone, *The Nature of Intelligence*, 1924, and K. Lewin, "Vorsatz, Wille, Bedürfnis," *Psychol. Forschung*, 1926, *7*, pp. 330 f.

(95) Cf. McDougall, *OP*, pp. 99, 106, 110, and our note *91*.

(96) Cf. Groos, *PM*, p. 146.

(97) Cf. W. McDougall, "Use and Abuse of Instinct in Social Psychology," *Jour. of Abn. and Soc. Psychol.*, 1921–2, *16*, pp. 293, 325, 330; *OP*, pp. 110, 128, 314 f.; Lloyd Morgan, "Instinctive Behaviour and Enjoyment," *British Journal of Psychology*, 1921, *12*, pp. 1 f.

(98) K. Lewin, "Vorbemerkungen über die psychischen Kräfte und Energien, und über die Struktur der Seele," *Psychol. Forschung*, 1926, *7*, pp. 309 f.

(99) Analogous descriptions may be found in K. Lewin's war experience, referred to, and in part described, on p. 322 of this text.

(100) *PhG*, p. xiii. Counterparts of our concepts of end- and transitional situations are found by the vitalists in the morphologico-physiological domain. Thus Driesch distinguishes between "completeness" and "incompleteness," and defines the former as "allowing no sequential processes to take place from *internal* causes without a co-ordinate disturbance of form." Cf. H. Driesch, *Die organischen Regulationen, Vorbereitungen zu einer Theorie des Lebens*, Leipzig, 1901, p. 84.

(101) M. Wertheimer, "Experimentelle Studien über das Sehen von Bewegungen," *Ztschr. f. Psychol.*, 1912, *61*, pp. 251 f. (Frankf. Habil-Schr., p. 91). Now reprinted in *Drei Abhandlungen zur Gestalttheorie*, Erlangen, 1925.

(102) These conditions, which I have omitted from the text in order not to confuse readers who are not proficient in natural science, have been formulated by Köhler as follows (*PhG*, p. 250): "In every process which issues at all in an end-situation, independent of time, the mode of distribution shifts in the direction of a minimum of configurative energy." The last-mentioned considerations in the text all follow Köhler's book, the next citation in the text being from p. 257.

(103) K. Koffka, "Mental Development," *Pedagogical Seminary*, 1925, *32*, p. 666.

(104) Cf. the experiments of Marina, described in note *78*.

(105) Cf. Volkelt, pp. 15 f. E. Rabaud has recently published detailed observations on the behaviour of different species of spider. Unfortunately, it is impossible to consider them here, though they are of great interest, and agree in many ways with our view of instinctive behaviour and its materially conditioned limitations. The author does not appear to be acquainted with Volkelt's work, for he makes no reference to it; yet many of Volkelt's interpretations are touched upon. Cf. "Recherches expérimentales sur le comportement de diverses araignées," *Année psychologique*, 1922, *22*, pp. 21 f.

(106) Cf. Köhler's article, cited in note *78*, in which these matters are clearly and sharply discussed. The relation here accepted between structure and function seems to agree with the most recent results and inferences of modern biology. F. W. Gamble presents

the case simply and concretely in an address based upon Child's work. Gamble speaks of a "sedimentation" in organic life which increases with the intensity of life, and says that "stability of construction brings the penalty of diminished dynamic activity." (Cf. "Construction and Control in Animal Life," *Proceedings of the 92nd Meeting of the British Association for the Advancement of Science, Toronto*, 1924, pp. 109 f.; also Charles M. Child: *Physiological Foundations of Behaviour*, New York, 1924.) When, finally, an avowed behaviourist like Kuo excludes innate achievements altogether, although his position is not in all respects well taken, it serves, nevertheless, as a caution against any assumption of innate specialized bonds of connection as the explanation of each special achievement. (Cf. "A Psychology without Heredity," *Psychological Review*, 1924, *31*, pp. 427 f.). J. R. Kantor appears to hold similar views when he asks: "Does the neural apparatus control the muscles any more than the muscles and glands control the neural apparatus? Is it not a fact that the specific pathways involved in any reaction are involved because certain muscles or glands need to function?" Kantor answers that he "is firm in his disbelief in the functional priority of any system to any other." (Cf. "The Psychology of Reflex Action," *American Journal of Psychology*, 1922, *33*, p. 28.)

(107) *GS*, p. 401. In agreement is the discovery of C. J. Herrick and G. E. Coghill on the amphibian *Amblystoma*. Both functionally and morphologically, cruder and more diffuse reflexes precede finer and more differentiated reflexes. The short "two-neurone reflex" is certainly not present at the beginning, but only at the end of development. Cf. "The Development of Reflex-Mechanisms in Amblystoma," *Journal of Comparative Neurology*, 1915, *25*, pp. 65 f.

(108) "Zur Theorie der Funktion des Nervensystems," *Archiv. f. Psychiatrie u. Nervenkrankheiten*, 1925, *74*, pp. 370 f.

(109) Cf. my review of F. C. Bartlett's "Psychology and Primitive Culture," *Psychologische Forschung*, 1926, *7*, pp. 285–6.

(110) *PC*, p. 72. Stern remarks that what he describes as the instinct of attraction is a reaction of the same kind that Köhler observed in chimpanzees. Without reference to any practical end, the animal finds himself required to do something in the direction of a desired object. Cf. Köhler, *I*, p. 65 (89), and our citation on p. 20.

(111) *AG*, p. 46.

(112) Cf. Morgan, p. 41.

(113) Cf. Kafka, p. 466.

(114) Cf. Watson, *B*, pp. 125 f.

(115) *OP*, pp. 111 f.

(116) *I*, p. 58 (80).

(117) *I*, p. 68 (93).

(118) Thorndike, curiously enough, thinks it probable that ornamenting, tattooing, etc., are modes of behaviour acquired by successful achievements. Cf. *EP*, p. 140.

(119) Quoted from Thorndike, *EP*, p. 159 note. Here also belong the "warning cries" which motivate flight in large groups of animals. Lloyd Morgan has made an observation of especial interest in this connection. A duckling, still confined in the shell, peeps while it begins to make a hole in the shell. Whenever a warning cry is uttered by the hen, the duckling's reaction ceases. In comparison with other animals, it is quite possible that the expressive movements of man are "incomplete." In other words, these movements

originally were more far-reaching than now, and led to a change of environment or to an external achievement. In some ways our understanding of human expressive movements is aided by this assumption. But such an explanation is at best inadequate. In the first place, the original movements themselves must be explained; and, secondly, what does their transformation into the present "incomplete" movements signify? On both these points I can not insist too strongly that the prevailing "Darwinian" explanation is inadequate. Expressive movements simply can not have arisen by chance, and been retained on account of their "utility." One has only to read Julian Huxley's description of the courtship of birds (cf. *Essays of a Biologist*, London, 1923) in order to be convinced of the absurdity in such an hypothesis. Nor is this transformation to be explained on the ground of utility. On the other hand, the immediate intimacy of the relationship between the organism and its environment becomes much more obvious and direct when we cease to make the educated adult of western Europe the object of our observations, and instead, consider the behaviour of lower forms of life (cf. text pp. 1 and 2.)

(120) Cf. "Zur Psychologie des Schimpansen," *Psychologische Forschung*, 1922, *1*, pp. 27, 28, 31, 33 (translated in *The Mentality of Apes*, pp. 305 f.).

(121) Cf. Köhler, *Methoden* (note *11*), pp. 75 f., especially 79, and 100 f.

(122) Bühler, *GE*, p. 91.

(123) The ear seems to furnish an exception, since at birth the middle-ear is filled with fluid instead of air, so that no transfer of tone-stimulation to the sense-organ within the inner-ear can take place. All new-born infants are therefore deaf, but they react to sound as soon as the fluid has disappeared from the middle-ear.

(124) *PC*, pp. 73 f.

(125) Henry Head, *Studies in Neurology*, 2 vols., London, 1920.

(126) To my knowledge it has not yet been demonstrated whether the protective reflex-action of the *tensor tympani* (the drum-membrane muscle) is already functional; and such a demonstration might be difficult to make, but probabilities favour the conclusion that it is functional. Cf. for the following: the papers of Hetzer and Tudor-Hart, and B. Löwenfeld referred to in note *79*, and Charlotte Bühler: "Die ersten sozialen Verhaltungsweisen des Kindes," *Quellen und Studien zur Jugendkunde*, Heft 5, pp. 1 f.

(127) *GE*, 1st ed. (1918), p. 355.

(128) *EP*, pp. 301 f.

(129) Thorndike, to be sure, encounters difficulties. He reckons among the original tendencies those that make modifiability or learning possible; but at the same time he defines all inherited dispositions as bonds between neurones. Thorndike wishes to recognize plasticity, but in order to do so, he enlarges his concept of inherited disposition, without appearing to realize that he has done this (cf. *EP*, vol. 2, p. 1).

(130) Cf. with this the discussion of Volkelt with reference to "learning capacity" and "incapacity to learn," pp. 120 f.

(131) I, p. 295.

(132) Cf. Stern's discussion of the primitive unity of the sensomotorium, *PC*, pp. 61 f.

(133) Cf. in this connection Köhler, *PhG*, pp. 57 f., 192, 207. The phenomenal and functional differences between figure and ground have been treated in detail in a monograph by E. Rubin:

Synsoplevede Figurer, Köbenhavn og Kristiania, 1915. (German ed.: *Visuell wahrgenommene Figuren*, Copenhagen, 1921.)

(134) This view is also rejected by Stern (*PC*, pp. 102 f.); yet there are essential differences between his view and our own. We can neither accept Stern's concept of sensation (cf. the discussion of clang-analysis on p. 238, and my essay, referred to in note 2), nor can we admit that mere "diffuseness" is an adequate description of the original state of consciousness. Concerning the principles involved, cf. Wertheimer's "Untersuchungen zur Lehre von der Gestalt, I, Prinzipielle Bemerkungen," *Psychologische Forschung*, 1921, *1*, pp. 47 f.

(135) This fact is emphasized particularly by Mrs. Moore. For example, p. 151.

(136) Stern, *PC*, p. 108, Bühler, Hetzer and Tudor-Hart, etc.

(137) *StF*, p. 49 (note).

(138) *CP*, pp. 291 and 271.

(139) H. Driesch: The Crisis in Psychology, Princeton, 1925, pp. 107 f.

(140) Cf. Scheler, *Sympathie* (note *16*), p. 275, also Köhler's discussion based upon his observations and experiments upon chimpanzees, *Psychologische Forschung*, 1921, *1*, pp. 37 f.

(141) Cf. for instance, Stern, *PC*, pp. 75 f., and Guillaume, *l'Imitation, loc. cit.*, p. 144.

(142) Cf. Lévy-Bruhl, pp. 27 f., and T. W. Danzel, "Prinzipien und Methoden der Entwicklungspsychologie." Abderhalden's *Handbuch der biol. Arbeitsmethoden.*, Lief. 46, Abt. VI, e, Berlin and Vienna, 1921; also my review in *Psychologische Forschung*, 1923, *3*, pp. 189 f.

(143) M. Brod and F. Weltsch, *Anschauung und Begriff*, Leipzig, 1913, p. 6.

(144) So Wertheimer reported at the 5th Congress for experimental psychology in Berlin, 1912.

(145) The method employed by Katz and Révész, in which the "grain denied to the fowl" was simply glued fast to the plate, can not be employed with large and powerful birds, which are able to tear the grain loose. Cf. Köhler, *OU*, p. 58.

(146) Köhler, *StF*, pp. 12–13.

(147) Helene Frank has repeated these experiments with six children between the ages of eleven months and seven years, substituting difference of size for difference of brightness. Cf. "Untersuchungen uber Sehgrössenkonstanz bei Kindern, *Psychologische Forschung*, 1926, *7*, pp. 137 f.

(148) *StF*, p. 24.

(149) Cf. A. Riekel, "Psychologische Untersuchungen an Hühnern," *Zeitschr. f. Psychol.*, 1922, *89*.

(150) Köhler's results have been subsequently verified with different animals. All the references are given by E. C. Tolman in his excellent report on "Habit Formation and Higher Mental Processes in Animals," *Psychol. Bulletin*, 1927, *24*, pp. 14 f. An experiment carried out by one of Tolman's pupils is worthy of special mention, because it reveals the tremendous effectiveness of the structural choice as compared with the "absolute" choice in the case of albino rats. In this experiment the original choice was made in a discrimination-box, and it consisted in avoiding a dark alley (A) and running into one that was illuminated by a 60-Watt

lamp (B). In the critical tests which followed training, B was retained, and instead of A there was substituted an alley illuminated by a 100-Watt lamp (C). The choice was then no longer between darkness and light, but between two lighted alleys, one brighter than the other. Despite this fact C was chosen in a majority of cases, although B was not infrequently chosen. Cf. A. H. Gayton, "The Discrimination of Relative and Absolute Stimuli by Albino Rats," *Journal of Comp. Psychol.*, 1927, 7, pp. 93 f.

(151) D. Katz, "Der Aufbau der Tastwelt," Erg. Band *11* der *Zeitschr. f. Psychol.*, 1925, pp. 71 f.

(152) Cf. M. Wertheimer, *Zeitschr. f. Psychol.*, 1912, *61*, and various of my "Beiträge zur Psychologie der Gestalt" in the same periodical, and in *Psychol. Forschung*.

NOTES TO CHAPTER IV

(153) Katz, *Tastwelt* (note *151*).

(154) Goldstein, K., "Zur Theorie der Funktion des Nervensystems," *Archiv. für Psychiatrie*, 1925, *74*, pp. 370 f.

(155) Claparède makes a similar distinction between "méchanisme de la genèse" and "méchanisme de l'exercice" (French edit., p. 173).

(156) Cf. for example Thorndike, *EP*, pp. 25, 201.

(157) Exact descriptions of the conditions of the experiment may be found in Thorndike, *AI*, and in Watson, *B*.

(158) An arrangement of the opposite kind is also possible, where the food is in the box and the animal outside; but this does not in any way alter the principle. Cf. Watson, *B*.

(159) p. 165.

(160) *GE*, pp. 6, 119 f.

(161) *GE*, 2nd ed., p. 209. This sentence is omitted from the fourth edition.

(162) *AI*, pp. 108 f.

(163) *GE*, p. 6.

(164) *AI*, pp. 108 f., and *EP*, vol. 2, pp. 1 f.

(165) Watson, *B*, pp. 186, 259–60.

(166) Cf. the following with Watson, *B*, pp. 262 f. In his second book, Watson expresses himself with greater restraint. After discussing four theoretical possibilities, he recognizes the hypothetical character of the whole matter and refrains from accepting any one of them. *PB*, pp. 315 f.

(167) Cf. Thorndike, *EP*, pp. 185 f.

(168) Cf. Watson, *B*, p. 257, and note *164*.

(169) Thorndike subsumes under this law two groups of facts: that a bond is strengthened by use, and that it is weakened by long disuse. *EP*, pp. 171 f.

(170) *GE*, p. 120.

(171) According to Stout, in *A Manual of Psychology*, 3rd ed., London, 1913, p. 382. Cf. in general, Stout's keen criticism of Thorndike's theory. Examples like those cited in the text will also be found in Thorndike, *AI*, p. 72.

(172) Carl J. Warden and Edna L. Haas, "The Effect of Short

Intervals of Delay in Feeding upon Speed of Maze-Learning.' *Journal of Comp. Psychol.*, 1927, *7*, pp. 107 f.

(173) *EP*, pp. 188 f.

(174) *EP*, pp. 172 f. Cf. also *EP*, vol. 2, pp. 1 f., where further peculiarities of learning are discussed from the same point of view. The law of effect is flatly contradicted by the results of Kuo's experiments, in which rats learned to choose the one of two ways contrary to the law of frequency, and despite the fact that the rejected way led to success. Cf. "The Nature of Unsuccessful Acts and their Order of Elimination in Animal Learning." *Journal of Comparative Psychology*, 1922, *2*, pp. 1 f.

(175) *EP*, pp. 281–2.

(176) *AI*, pp. 43–4.

(177) Lloyd Morgan, pp. 152–3.

(178) *AI*, p. 119.

(179) Cf. with this, Köhler, *I*, pp. 16 f. (22 f.).

(180) Glenn D. Higginson, "Visual Perception in the White Rat," *Journal of Exper. Psychol.*, 1926, *9*, pp. 337 f.

(181) Not all the curves have been reproduced. One of an animal which was first tested in the wooden-latch box appears more like the second curves reproduced by us in Fig. 8.

(182) *AI*, p. 48. The italics are mine.

(183) *AI*, p. 119.

(184) Cf. with this *AI*, pp. 117 f.; and Köhler, *I*, pp. 8 (11) 130 (180 f.).

(185) Köhler, *I*, p. 17 (23).

(186) Thorndike, *AI*, p. 48.

(187) *OP*, pp. 196 f.

(188) *Psychological Bulletin*, 1927, *24*, p. 25.

(189) H. A. Ruger, "The Psychology of Efficiency," *Archives of Psychology*, Nr. 15, 1910.

(190) *Ibid.*, p. 9.

(191) A similar interpretation of learning by trial and error has been given by E. C. Tolman. Cf. *Psychological Bulletin*, 1927, *24*, p. 27.

(192) Köhler, *I*.

(193) *I*, p. 3 (4).

(194) Recently also J. Peiser, "Prüfungen höherer Gehirnfunktionen bei Kleinkindern," *Jhb. f. Kinderheilkunde und physische Erziehung*, 1920, *91*, pp. 182 f., and H. Bogen in the book cited in note 27.

(195) *I*, pp. 6 f. (8 f.).

(196) *I*, p. 19 (27). The same results were obtained in similar experiments made by W. T. Shepherd. Dogs and cats can not make use of string-connections (or even simpler relations), but Rhesus monkeys—which do not belong to the anthropoids—have no such difficulty. Cf. "Tests on Adaptive Intelligence in Rhesus Monkeys," *Amer. Jour. of Psych.*, 1915, *26*. An elaborate account of experiments of this order on a dog was published by A. Franken in 1911 (cf. "Instinkt und Intelligenz eines Hundes," *Zeitschrift für angewandte Psychologie*, *4*). The dog in question learned the string-connection in some degree; precisely how is not clear. Cf. Lipmann and Bogen, pp. 13 and 15.

Similar results, negative for dogs and positive for monkeys, have

been secured by various investigators. Cf. Tolman, *Psychol. Bulletin*, *24*; also H. Nellmann and W. Trendelenburg, "Ein Beitrag zur Intelligenzprüfung niederer Affen," *Zeitschr. f. vergl. Physiologie*, 1926, *4*, pp. 180 f.

(197) Cf. similar experiments upon monkeys performed by Nellmann and Trendelenburg, *loc. cit.*, pp. 152 f.

(198) *I*, p. 28 (39 f.).

(199) *I*, p. 25 (35).

(200) *I*, p. 25 (35).

(201) Bühler, *GE*, pp. 22 f. The reader may compare the discussion in the text with Bühler's new Preface (especially p. x), in which my interpretation is criticized, and then draw his own conclusions.

(202) Cf. Tolman, *loc. cit.*, and Nellmann and Trendelenburg, *loc. cit.*, p. 155 f.

(203) Köhler, *I*, pp. 30 f. (41 f.).

(204) *I*, pp. 30 f. (41 f.).

(205) One can not interpret this behaviour as indicating merely that the box was previously too heavy for the animal, because in an earlier experiment the ape had actually pushed a box upon which Tercera was lying; but the problem then was only one of removing an obstacle. Cf. *I*, pp. 128 f. 178 f.).

(206) Of the two monkeys tested by Nellmann and Trendelenburg, the Pavian found this task very difficult, while the Rhesus monkey found it easy (*loc. cit.*, p. 169). Further experiments are needed in order to determine under what conditions this achievement is easy or difficult.

(207) *I*, pp. 91 f. (127 f.).

(208) *I*, pp. 96 f. (135 f.).

(209) *I*, p. 166 (229).

(210) Cf. also *I*, pp. 175 (241 f.)

(211) *I*, p. 180 (249).

(212) *I*, p. 138 (192)

(213) *AI*, p. 192.

(214) *I*, pp. 140 f. (195 f.). Cf. with this also the above-mentioned results of Ruger (pp. 193 f.).

(215) *I*, p. 112 (157).

(216) *I*, p. 170 (235). According to the general plan of his work, Köhler leaves the decision open between the explanation we have made in the text and some other possibility. But after his later publications, *StF*, and *PhG*, there can be no doubt as to which possibility he really accepts.

(217) A similar interpretation of the creative process of learning is given by R. M. Ogden in his book, entitled "Psychology and Education," 1926, pp. 244 f. Ogden's terms are: for unification, *assimilation*; for analysis, *differentiation*; for *articulation*, gradation; and for re-articulation, *re-definition*. I have not adopted his terminology because, while he proposes a general theory of learning, I have been content to point out certain analogous features appropriate to a description of the creative process as it occurs in the experiments reported by Köhler. There is, however, no necessary conflict between the general interpretation Ogden offers, and the more restricted one which I am suggesting.

(218) In presenting my views of the learning process in the first edition of this book, I was largely influenced by the experiments of

Köhler upon chimpanzees. At that time these experiments had not yet won the recognition which is now accorded to them. The first reaction of psychologists to Köhler's results was very cautious. While every one praised the ingenuity of his work, few were ready to accept his interpretations. These interpretations were so different from prevailing views that several psychologists undertook to bring his results into harmony with current theories of learning. It therefore seemed expedient for me to give a full discussion of these criticisms and amendments of Köhler's work in order to make it plain that in my elaboration of his results I was not shirking the issue of their interpretation. Thus, in the first editions of this book, and also in the second German edition, two long sections were devoted to various objections which had been raised in different quarters. Now the situation has changed. Köhler's experiments have been repeated and confirmed by different investigators; other materials pointing to the same conclusions have become available; and the theoretical issues have been widely debated in technical journals (cf. especially, W. Köhler, "Zur Theorie der Sukzessivvergleichs und der Zeitfehler." *Psychologische Forschung*, 1923, *4*). For these reasons I have omitted most of my previous arguments, excepting those which bear directly upon some general principle.

(219) *GE*, pp. 10 f.; *AG*, pp. 16 f. Cf. with this *GE*, p. 408 and the new Preface, p. x.

(220) *Stimmen der Zeit*, 1918, *95*.

(221) O. Selz, *Über die Gesetze des geordneten Denkverlaufs*, II, Stuttgart, 1922.

(222) 2nd ed., p. 20.

(223) 4th ed., p. 21. The citations in the text are made as full as possible, because Bühler has objected to my presentation of his views. The interested reader is advised to compare Bühler's objections (p. ix) with my text. I would remind him, however, that in note *181* of the previous edition of my book I wrote: "I do not believe that Bühler still maintains this doctrine in all strictness." I also referred to Bühler's chapter in which he "develops quite a different theory of perceived relations." It was not to my purpose to enter into a discussion of his theory, which did not seem relevant enough to demand my criticism. Köhler, however, has refuted the theory in his article: "Zur Theorie des Sukzessivvergleichs und der Zeitfehler" (*Psychologische Forschung*, 1923, *4*, pp. 115 f., especially, pp. 125 f.). I did refer, without a special citation, to a statement which in the 2nd ed. of Bühler's book (p. 358) reads as follows: "All acceptable views of the matter must be in harmony at least with the assumption that material relations can be perceived and 'noted' without a true experience of judgment necessarily being present." This statement reappears in the 4th ed. (p. 374), with an important modification. Instead of presupposing "harmony," we are invited to inquire into the validity of this assumption.

(224) Cf. also Bühler, *Die Gestaltwahrnehmungen*, I, Stuttgart, 1913, pp. 16 f.

(225) The doctrine of unnoticed sensations, which finds its classical expression in the first volume of Stumpf's *Tonpsychologie* (1883), was attacked by Cornelius in his *Psychologie als Erfahrungswissenschaft* (1897). More recently Köhler has written a paper bearing directly upon this subject: "Über unbemerkte Empfindungen und Urteilstäuschungen," *Ztschr. f. Psychol.*, 1913, *66*. Cf.

also my paper: "Probleme der experimentellen Psychologie," in *Die Naturwissenschaften*, 1917, *5*, Nos. 1 and 2.

(226) *GE*, pp. 21, 372 f.

(227) For a fuller treatment of the fundamental question involved in this discussion, see my article referred to in note *272*. The quotation from Lindworsky is taken from *Stimmen der Zeit*, 1918, *95*, p. 391.

(228) Cf. the following with Köhler, *StF*, pp. 56 f.

(229) *Stimmen der Zeit*, 1919, *97*, p. 66.

(230) "Einige Allgemeinere Fragen der Psychologie und Biologie des Denkens, erläutert an der Lehre vom Vergleich," *Arb. z. Psychol. u. Phil.*, edited by E. R. Jaensch, I, Leipzig, 1920. Apparently Köhler's publication was unknown to Jaensch, since he does not mention it. Cf. the work of Jaensch's pupil, A. Riekel, "Psychologische Untersuchungen an Hühnern," *Zeitschrift für Psychologie*, 1922, *89*, pp. 81 f. In a recent theoretical paper on the foundations of his psychology, Lindworsky accepts physiological functions of configuration, to which he ascribes the achievements of choice-training; but he still denies any consciousness of relations or insight in this behaviour (cf. "Umrisskizze zu einer theoretischen Psychologie," *Zeitschrift für Psychologie*, 1922, *89*, pp. 313 f., especially p. 343, note. The paper is also printed separately, 2nd ed., Leipzig, J. A. Barth). Köhler has given answer to Jaensch, Riekel, Bühler and Lindworsky in the article mentioned in note *223*, together with a well-knit theory of the perception of relations. I can not enter here into a discussion of Lindworsky's fundamental point of view. I have discussed it fully in my essay on Psychology, and have there shown why I regard it untenable.

(231) Köhler, *StF*, p. 13. We have already given the full quotation on p. 156.

(232) Jaensch, *op. cit.*, p. 24. Cf. Bühler, *GE*, p. 183, and the similar statements of Lindworsky, *Stimmen der Zeit*, *97*, pp. 64 f.

(233) Jaensch, *op. cit.*, p. 21.

(234) According to Jaensch (p. 20), transitional experiences are "of the same kind as the phenomena of movement described by Linke, Wertheimer, and Koffka." But these phenomena of movement are from our point of view only typical phenomena of configuration. Cf. also Wertheimer's work cited in note *101*.

(235) *Op. cit.*, p. 28.

(236) Cf. with this Köhler's discussion in the work cited in note *223*.

(237) *GE*, p. 187.

(238) Cf. Köhler's "Akustische Untersuchungen," especially III and IV, "Vorläufige Mittlg.," *Ztschr. f. Psychol.*, 1913, *64*, pp. 99 f. and III, *ibid.*, 1915, *72*, pp. 121 f; also Margarete Eberhardt, "Über die phänomenale Höhe und Stärke von Teiltönen," *Psychologische Forschung*, 1922, *2*, pp. 346 f.

(239) This law cuts across another; viz. that *ceteris paribus*, the colour-threshold is lower for a dark field than for a bright one. The fact reported in the text was first pointed out by Stumpf ("Die Attribute der Gesichtsempfindungen," *Abhdlg. d. K. Preuss. Ak. d. Wiss.*, Jhrg., 1917, *Phil.-Hist. Kl.*, Nr 8, p. 84 f., which includes references to earlier investigations of the subject). I myself reported an investigation before the *Naturforscher-Versammlung* at Nauheim (1920), which substantiates and supplements Stumpf's conclusions, and I employed then the point of view of the psychology of con-

figuration. These experiments have since been carried forward and published. (Cf. A. Ackermann, "Farbschwelle und Feldstruktur," and M. Eberhardt, "Untersuchungen über Farbschwellen und Farbenkontrast," both in *Psychologische Forschung*, 1924, *5*, pp. 24 f., and 44 f.). Even the differential threshold for brightness is dependent upon the brightness of the field surrounding the surface to be tested, and the threshold is minimal when background and surface tested have the same degree of brightness. This was demonstrated by F. Dittmers in the Göttingen laboratory ("Über die Abhängigkeit der Unterschiedsschwelle für Helligkeiten von der antagonistischen Induktion," *Ztschr. f. Sinnesphysiol.*, 1920, *51*. M. Eberhardt has reviewed the entire subject in a summary printed in *Psychologische Forschung*, *5*. But to find here a configurative uniformity does not mean that an exact physico-chemical explanation must be given up, and the advantages of the Müller colour-theory abandoned; for configurative uniformities are also physico-chemical, a statement which is hardly necessary to readers of Köhler's book on physical configurations.

(240) Certainly it does not agree with the description Köhler gives (*OU*, pp. 59–60) of learning in hens.

(241) Köhler, *I*, p. 92 (129).

(242) *Stimmen der Zeit*, 1919, *97*, p. 66.

(243) *GE*, pp. 2 f. Cf. also p. 408.

(244) Cf. Bühler, *GE*, p. 50.

(245) *GE*, pp. 342 and 447 f.

(246) Köhler, *StF*, p. 51, also his discussion of the same point in *Pedagogical Seminary*, *32*, pp. 678 f.

(247) *StF*, pp. 85–6.

(248) Cf. G. E. Müller, "Zur Analyse der Gedächtnistätigkeit und des Vorstellungsverlaufs," I. Erg.-Bd. *5*, d. *Ztschr. f. Psychol.*, 1911, pp. 332 f., 372.

(249) *Ibid.*, III, Erg.-Bd. *8*, 1913, pp. 210 f.

(250) Cf. A. Aall, "Ein neues Gedächtnisgesetz," *Ztschr. f. Psychol.*, 1913, *68*, pp. 43 f.

(251) Cf. A. Kühn, "Über Einprägung durch Lesen und Rezitieren," *Ztschr. f. Psychol.*, 1914, *68*, pp. 396 f., especially pp. 443, 473 f.

(252) Cf. K. Lewin, "Die psychische Tätigkeit bei der Hemmung von Willensvorgängen und das Grundgesetz der Assoziation," *Ztschr. f. Psychol.*, 1917, *77*, p. 245; and "Das Problem der Willensmessung und das Grundgesetz der Assoziation," *Psychol. Forschung*, 1922, *1* and *2*, pp. 191 f., and 65 f.

(253) Cf. J. von Kries, *Über die materiellen Grundlagen der Bewusstseins-Erscheinungen*, Tübingen und Leipzig, 1901; and Becher, *GS*, pp. 161–327.

(254) *Op. cit.*, pp. 41–42.

(255) *GS*, pp. 284 f.

(256) For a concise summary of Lashley's work, including the various references, cf. E. C. Tolman, *Psychol. Bull.*, 1927, *24*, p. 6.

(257) The complacency with which the American behaviour-psychologists ignore this criticism is truly astounding.

(258) Cf. E. Rabaud, "Recherches expérimentales sur le comportement de diverses araignées," *Année psychol.*, 1922, *22*, for instance, pp. 50 f., and especially the conclusion, pp. 56 f.; also the Peckhams, *Wasps, Social and Solitary*, Boston, 1905.

(259) This statement is paraphrased from one in my article entitled "Mental Development," *Pedagogical Seminary*, 1925, *32*, p. 673.

NOTES TO CHAPTER V

(260) Claparède speaks in such cases of "familiar knowledge," *avoir familier* (French ed., pp. 190 f.).

(261) In the article cited in note *120*, p. 6, and *The Mentality of Apes*, p. 276.

(262) *PC*, p. 110. Cf. also Groos, *SK*, p. 34.

(263) Cf. the preceding with *PC*, p. 110, and Shinn, I, p. 22.

(264) Cf. also Bühler, *GE*, p. 318.

(265) Bühler, *GE*, p. 335.

(266) *EA*, pp. 3 f.; *PC*, pp. 238 f.; Bühler, *GE*, pp. 314 f.

(267) Cf. Stern, *PC*, p. 287, also Köhler, *I*, pp. 56 f.(77 f.), and his article referred to in note *120*, pp. 2 f., and *The Mentality of Apes*, pp. 272 f.

(268) Cf. E. R. Jaensch, "Die experimentelle Analyse der Anschauungsbilder als Hilfsmittel zur Untersuchung der Wahrnehmungs- und Denkvorgänge," *Sitz.-Ber. d. Ges. z. Bef. d. Ges. Naturwiss*, zu Marburg, 1917, No. 5. The same: "Zur Methodik experimenteller Untersuchungen an optischen Anschauungsbildern," *Ztschr. f. Psychol.*, 1920, *85*. Paula Busse, "Über die Gedächtnisstufen und ihre Beziehung zum Aufbau der Wahrnehmungswelt," *Ztschr. f. Psychol.*, 1920, *84*. A vast amount of work has since been published by Jaensch and his students, a comprehensive view of which may be had from Jaensch's "Über die subjektiven Anschauungsbilder," *Bericht über d. VII Kongress f. Exper. Psychol. in Marburg*, Jena, 1922, pp. 3–49; from O. Kroh, *Subjektive Anschauungsbilder der Jugendlichen*, Göttingen, 1922, and from my critical report in the *Psychol. Forschung*, 1923, *3*, pp. 124 f. Jaensch's papers, which have appeared in journals, are now collected in book-form.

(269) Jaensch, *Sitz.-Ber.*, pp. 64-5.

(270) Busse, *op. cit.*, pp. 43 f. Concerning the theory, cf. my summary referred to in note *268*, and G. W. Allport, "Eidetic Imagery," *British Journal of Psychology*, 1924, *15*, pp. 99 f., especially, pp. 114 f. and 120.

(271) *Sp*, pp. 362 f.

(272) O. Selz, *Über die Gesetze des geordneten Denkverlaufs*, I, Stuttgart, 1913; II, Stuttgart, 1922. A critical review by Benary will be found in *Psychologische Forschung*, 1923, *3*, pp. 417 f. Selz has defended his views against my criticism, and I have replied with a full and detailed discussion. Cf. O. Selz, "Zur Psychologie der Gegenwart. Eine Anmerkung zu Koffka's Darstellung," *Zeitschrift f. Psychol.*, 1926, *99*, pp. 160 f., and K. Koffka, "Bemerkungen zur Denk-Psychologie," *Psychol. Forschung*, 1927, *9*, pp. 162 f.

(273) L. Schlüter, "Experimentalle Beiträge zur Prüfung der Anschauungs- und Übersetzungsmethode bei der Einführung in einen fremdsprachlichen Wortschatz," *Ztschr. f. Psychol.*, 1914, *68*, pp. 103 f. Cf. also the chapter on association by similarity in my book *Zur Analyse der Vorstellungen und ihrer Gesetze*, Leipzig, 1912, pp. 343–360.

(274) R. Heine, "Über Wiedererkennen und rückwirkende Hemmung," *Ztschr. f. Psychol.*, 1914, *68*.

(275) In the book just cited, pp. 344 f., which also contains a bibliography. Cf. also Wertheimer, *op. cit.*, p. 252 (note). In his second volume (cf. note *272*) Selz opposes a direct reproduction by similarity.

(276) Cf. Watson, *B*, pp. 138 f., and for the following paragraph, Dorothy Moseley, "The Accuracy of the Pecking Response in Chicks," *Journal of Comparative Psychol.*, 1925, *5*, pp. 75 f.

(277) Cf. Compayré, I, pp. 287 f.

(278) Cf. Guillaume, *l'Imitation*, *loc. cit.*, pp. 76 f.

(279) Preyer, I, pp. 241 f.; Shinn, I, pp. 306 f.; Watson, *PB*; and G. C. Myers, "Grasping, Reaching, and Handling," *American Journal of Psychol.*, 1915, *26*, pp. 525 f.

(280) In the article cited in note *52*, p. 121.

(281) *GE*, p. 120. Cf. also p. 8.

(282) *GE*, p. 109.

(283) *PC*, p. 117.

(284) *Op. cit.*, pp. 123 f.

(285) *Op. cit.*, (note *253*), pp. 21, 32 f.

(286) Shinn, I, pp. 306–7.

(287) Compayré, II, pp. 9 f. Cf. also Preyer, I, pp. 283 f.

(288) *Sp*, p. 15; *PC*, p. 91. Cf. also Bühler, *GE*, pp. 216 f.

(289) L. E. Ordahl, "Consciousness in Relation to Learning," *Amer. Jour. of Psychol.*, 1911, *22*, p. 189.

(290) E. C. Rowe, "Voluntary Movement," *Amer. Jour. of Psychol.*, 1910, *21*, p. 331.

(291) W. Betz, *Psychologie des Denkens*, Leipzig, 1918, pp. 48 f.

(292) *Op. cit.*, pp. 181 f.

(293) *I*, p. 126 (176), note.

(294) Stern, *PC*, p. 82. Concerning volition, cf. my essay on Psychology, referred to in note *2*.

(295) Stern, *PC*, p. 188.

(296) C. W. Valentine, "The Colour Perception and Colour Preferences of an Infant during its fourth and eighth months." *British Journal of Psychology*, 1914, *6*, pp. 363 f.

(297) W. A. Holden and K. K. Bosse, "The Order of Development of Colour Perception and Colour Preference in the Child," *Archives of Ophthalmology*, 1900, *29*, pp. 261 f.

(298) *Sp*, p. 229. The italics are mine.

(299) The Sterns write (*Sp*, p. 229) "that the difference between variegated and non-variegated colours is much more striking and important to the child than the difference between the different colours themselves." Their explanation in terms of attention and lack of interest, instead of in terms of sensory factors, is refuted in our text.

(300) Of the protanopic type (red-weak).

(301) Normal observers report similar phenomena near the region of the threshold. Cf., for instance, Ackermann's work cited in note *239*, pp. 51 f.

(302) Cf. A. Binet, "Perceptions d'Enfants," *Rev. Philos.*, 1890, *30*, and W. H. Winch, "Colour-names of English School Children," *Amer. Jour. of Psychol.*, 1910, *21*.

(303) Preyer also recognizes (I, p. 21) that errors of this sort can not be altogether a matter of naming.

(304) Bühler, *GE*, p. 194.

(305) Köhler, *StF*, pp. 67–72. With the pair AB, B was chosen 19 times out of 20, and with the pair DE, E was selected in each of the 21 trials.

(306) Since 1892 Mrs. Ladd-Franklin has defended an "Evolution-theory of Colour-sensation," which, if we confine ourselves to its main features, is intimately related to the hypothesis here presented. However, there is one difference. According to Mrs Ladd-Franklin's theory, the four main colours did not arise symmetrically out of the warm—cold stage, that is, the warm colours yielding red and yellow, and the cold ones green and blue. Instead, just as the original system yielding only neutral light-sensations gave rise to the warm—cold distinction, so the warm side alone gave rise to the two new colours, red and green. This theory, which has lately been expressed independently by the physicist, E. Schrödinger, has much in its favour, and could, I think, be easily reconciled with my text. (Cf. C. Ladd-Franklin, "Colour-sensation Theory," *American Encyclopedia of Ophthalmology*, and E. Schrödinger: "Über das Verhältnis der Vierfarben- zur Dreifarbentheorie," *Wiener Berichte, Math.-nat. Kl. Abt. IIa*, 1925, *134*, pp. 471 f.

(307) Stumpf, *Spe*, p. 20.

(308) W. Peters, "Zur Entwicklung der Farbenwahrnehmung nach Versuchen an abnormen Kindern," *Fortschr. d. Psychol.*, 1915, *3*, pp. 152–3.

(309) *Op. cit.*, pp. 161–2.

(310) The first part of the conclusion has not been completely demonstrated either, since no tests were made with violet samples. The erroneous classification of purple occurred only with the purple and not with the red sample. An analogous behaviour with the violet sample would therefore be *a priori* possible.

(311) A. Gelb and K. Goldstein, "Über Farbennamenamnesie," *Psychologische Forschung*, 1925, *6*, pp. 127 f. The reference is to p. 152.

(312) D. Katz, "Die Erscheinungsweisen der Farben und ihre Beeinflussung durch die individuelle Erfahrung," Erg.-Bd., 7, d. *Zeitschr. f. Psychol.*, 1911.

(313) A. Gelb, "Über den Wegfall der Wahrnehmung von 'Oberflächenfarben.'" In *Psychologische Analysen hirnpathologischer Fälle*, 1920, *1*, p. 408. (Also in *Zeitschr. f. Psychol.*, 1920, *84*, p. 247.)

(314) *OU*, p. 39 f.; and "Die Farben der Sehdinge beim Schimpansen und beim Haushuhn," *Ztschr. f. Psychol.*, 1917, 77.

(315) Cf. Compayré, I, p. 104; Stern, *PC*, pp. 114 f.

(316) Details concerning these facts may be found in both of Jaensch's books, Erg.-Bde., *4* and *6* d. *Ztschr. f. Psychol.*, 1909 and 1911; and also in M. Jacobson's "Über die Erkennbarkeit optischer Figuren bei gleichem Netzhautbild und verschiedener scheinbarer Grösse," *Ztschr. f. Psychol.*, 1917, *77*. In addition, there is the interesting experiment reported in 1921 by Gelb at the 7th Congress for Experimental Psychology (cf. the *Kongressbericht*, Jena, 1922).

(317) Cf. Katz, *op. cit.*, p. 97.

(318) Cf. Stern, *PC*, pp. 115 f.; Bühler, *GE*, p. 353. In another place (p. 154), to be sure, Bühler expresses himself much more cautiously.

(319) *OU*, pp. 18 f. A theoretical discussion is also given here.

(320) W. Götz, "Experimentelle Untersuchungen zum Problem der Sehgössenkonstanz beim Haushuhn, *Zts. f. Psychol.*, 1926, *99*, pp. 247 f. Cf. Helene Frank, "Untersuchungen über Sehgrössenkonstanz bei Kindern," *Psychol. Forschung*, 1926, *7*, pp. 137 f. F. Beyrl, "Über die Grössenauffassung bei Kindern, *Zts. f. Psychol.*, 1926, *100*, pp. 344 f., and H. Frank, "Die Sehgrössenkonstanz bei Kindern, *Psychol. Forschung*, 1927, *10*, No. 1, p. 102 f.

(321) Cf. Jaensch's books cited in note *316*, and Erna Schur, "Mondtäuschung und Sehgrössenkonstanz," *Psychol. Forschung*, 1926, *7*, pp. 44 f.

(322) That stimulation of the eyes by light is necessary for the development of the optical centres is shown by Claparède's report (pp. 126 f.), that the visual centres of cats whose eyelids were sewn together at birth were arrested in their development. Cf. also Becher, *GS*, pp. 177 f.

(323) *PC*, p. 121.

(324) *Op. cit.* (note *268*), p. 59. Cf. also my critique cited in the same note.

(325) This conclusion has been verified in the experiments on after-images by A. Noll, "Versuche über Nachbilder," *Psychol. Forschung*, 1926, *8*, pp. 3 f.

(326) Cf. with this paragraph, K. Lohnert, "Untersuchungen über die Auffassung von Rechtecken," *Psychol. Studien*, 1913, *9*, E. Lenk, "Über die optische Auffassung geometrisch-regelmässiger Gestalten," *Neue Psychol. Studien*, 1926, *1*, W. Stern, *PC*, p. 189, and H. Volkelt, *Kongressbericht, loc. cit.*, pp. 93 f.

(327) *The Mentality of Apes*, p. 325.

(328) *PC*, p. 186. The perception of simple geometrical forms can be tested systematically with the aid of outlines, instead of by Miss Shinn's method. According to Preyer's statements (I, p. 65), the results might then be quite different and more favourable. Thus at the end of the second year a child called a sketched circle a "ring," a square a "window," a triangle a "roof," etc.

It may also be mentioned that in an investigation which Groos made with a five-year-old girl, he found that regularly formed figures were preferred to irregular figures—a result which deserves further investigation. Cf. *PM*, p. 62. Furthermore, attention is called to the preference of young children (second year) for simple arrangements.

(329) Pertinent is the following observation of Schjelderup-Ebbe, "Wild ducks of the same sex are so much alike that a person can not distinguish them by their faces . . . yet among themselves ducks recognize one another with the greatest ease, never making a mistake." To be sure, I can not agree with the author's explanation as a "fineness of perception for optical angles and forms." (Cf. "Das Leben der Wildenten in der Zeit der Paarung," *Psychologische Forschung*, 1923, *3*, p. 17).

(330) Cf. Guillaume, *op. cit.*, note *52*, p. 132.

(331) Lévy-Bruhl, pp. 188–9.

(332) According to Stern, *PC*, p. 361.

(333) *PC*, p. 190. Cf. also W. Stern, "Über verlagerte Raumformen," *Ztschr. f. angew. Psychol.*, 1909, *2*; and Bühler, *GE*, p. 156.

(334) Cf. F. Oetjen, "Die Bedeutung der Orientierung des Lesestoffs für das Lesen und der Orientierung von sinnlosen Formen für

das Wiedererkennen derselben," *Zeitschrift fürPsychologie*, 1925, *71*, pp. 321 f.

(335) K. E. Pechuël-Loesche, *Volkskunde von Loango*, 1907, pp. 76 f.

(336) *GE*, p. 86 f. Cf. also the statements by Betz, *op. cit.* (cf. note *291*), pp. 50 f.

(337) J. Wittmann, *Über das Sehen von Scheinbewegungen und Scheinkörpern.*, Leipzig, 1921. Tables 5 and 6.

(338) Cf. Wittmann, *op. cit.*, pp. 171 f.

(339) *GE*, p. 267.

(340) *Op. cit.*, pp. 162–171.

(341) Cf. Köhler, *PhG*, pp. 253 f.

(342) Cf. M. Wertheimer, " Untersuchungen zur Lehre von der Gestalt, II," *Psychologische Forschung*, 1923, *4*, p. 333.

(343) Cf. Stern, *PC*, pp. 113 f. To be sure, our point of view is very different from Stern's.

(344) Cf. R. M. Ogden, *Psychology and Education*, 1926, pp. 124 f.

(345) Michotte has devoted some interesting tachistoscopic experiments to this very problem. He distinguishes between *organization intuitive* and *signification*, and maintains that " we cannot consider the acquisition of significance (*prise de sens*) as a simple addition to the shape . . . the intuitive organization becomes an integral and constitutive part of a much more comprehensive whole." Cf. A. Michotte, " Rapport sur la Perception des Formes," *VIIIth International Congress of Psychology, Proceedings and Papers*, Groningen, 1927, pp. 166 f.

(346) *PB*, p. 278 f.

(347) C. L. Hull, " Quantitative Aspect of the Evolution of Concepts," *Psychological Monographs*, 1920, *28*, pp. 4 f.

(348) K. Lewin, " Kriegslandschaft," *Ztschr. f. angew. Psychol.*, 1917, *12*.

(349) Cf. Bühler, *GE*, pp. 219 f.; Stern, *Sp*, pp. 155, 269.

(350) Franken, *op. cit.*, p. 63. The experiment consisted of a single test with a string, the goal hanging high up over the rung of a ladder. At first the younger child failed, but after seeing the older child perform the act spontaneously, he succeeded.

(351) *GE*, pp. 82 f.; *AG*, pp. 50 f.

(352) *I*, p. 10 (14).

(353) *I*, p. 176 (244). Cf. also Chapter I, p. 20.

(354) Preyer, II, p. 12. Observations of the most primitive achievements of intelligence during the first year of life are given by Stern, *PC*, pp. 84 f.

(355) Cf. Thorndike, *AI*, pp. 89 f., and C. S. Berry, " An Experimental Study of Imitation in Cats," *Journal of Comparative Neurology and Psychol.*, 1908, *18*, p. 24; also a general discussion of this problem by Guillaume, *l'Imitation*, *loc. cit.*, pp. 118 f. For the following paragraph, cf. the same book, pp. 131 f.

(356) Cf. Morgan, p. 168 f.; Stern, *PC*, pp. 92 f.; Groos, *SK*, p. 52; Thorndike, *EP*, pp. 108 f.

(357) Cf. with this and with what follows, Stern, *PC*, p. 90; *Sp*, pp. 148 f.; J. Mark Baldwin, *Mental Development in the Child and the Race*, New York, 1895; and Guillaume, *l'Imitation*, *loc. cit.*, p. 39.

(358) Cf. Preyer, I, p. 90; Stumpf, *Tonpsychologie*, I (1883), pp. 293 f.; II (1890), pp. 553 f.

(359) Cf. Preyer, II, p. 257, and Stern, *PC*, pp. 143 f.

(360) Stern, *Sp*, p. 153; Guillaume, *l'Imitation, loc. cit.*, pp. 34 f.

(361) Claparède, p. 142.

(362) In the meaning of Köhler's book, *PhG*.

(363) Cf. Köhler, *Pedagogical Seminary*, *32*, pp. 685 f.

(364) Cf. Moore, p. 18.

(365) A confirmation of this principle is found in the behaviour of Stumpf's son, who suddenly repeated four short prayers correctly and "almost faultlessly" after he had been speaking only his own language for years with never a word of the mother-tongue. "That he should utter the words almost faultlessly after talking a strange language, so to speak, up to this time, is certainly noteworthy," writes Stumpf. Cf. *SpE*, p. 22.

(366) Cf. with this the statements of Groos, *PM*, p. 286.

(367) *L'Imitation, loc. cit.*, p. 48.

(368) *I*, p. 161 (222), and article in *Pedagogical Seminary*, 1925, *32*, pp. 686 f.

(369) *PC*, p. 145.

(370) Cf. Stern's summary, *Sp*, pp. 158 f., and with what follows, *ibid.*, pp. 175 f.

(371) *GE*, pp. 392 f.; and also simpler and clearer, I think, in *AG*, pp. 58 f.

(372) Cf. Stern, *PC*, p. 375; *Sp*, p. 178.

(373) *AG*, p. 59.

(374) Cf. also Sully, pp. 76–7.

(375) Cf. Lévy-Bruhl, pp. 407 f., and 198 f.; also a similar observation upon children by Sully, pp. 76–7.

(376) Cf. Leo Frobenius, *Paideuma*, Munich, 1921, p. 22; and Sully, *op. cit.*, pp. 55, 56.

(377) Cf. *RM*, pp. 38 f., 61 f., 64.

(378) E. M. v. Hornbostel, "Laut und Sinn," *Festschrift Meinhof*, Hamburg, 1927, pp. 329 f.

(379) *PC*, p. 158.

(380) Moore, p. 125; Stumpf, *SpE*, p. 25. (The italics in this quotation are mine.) Cf. also Sully's remarks upon "colour-hearing" in children, pp. 33 f. "Naming" has recently been studied experimentally by S. Fischer ("Über das Entstehen und Verstehen von Namen," *Archiv für die gesamte Psychologie*, 1921–2, *42*, *43*), and by D. Usnadze ("Ein Experimenteller Beitrag zum Problem der psychologischen Grundlagen der Namengebung," *Psychologische Forschung*, 1924, *5*). In Fischer's experiments nonsense words were learned as names for the (nonsensical) "signs of a mysterious science"; also for portraits. Usnadze allowed his observers to choose from a previously assembled list of nonsense words the most appropriate names for six nonsensical drawings. Usnadze, in particular, summarizes his results by stating that in general an object is not named by chance; but that many factors are responsible for the selection of a name, among which those involving an intrinsic connection are the most significant. These are: (*a*) the factor of configurative relationship, which is the experience of agreement between the individual configuration of the utterance and the object; (*b*) the emotional factor of agreement; and (*c*) the factor of

"depth," which depends upon a general impression that is hard to define (pp. 38 f.). Cf. also Fischer, *43*, p. 34, and Stern's discussion, *PC*, pp. 145 f. Other sources for naming are given by Stern in *PC*, pp. 147 f.

(381) Stern, *Sp*, pp. 172 f.; cf. also among others, Preyer, II, p. 86, and Moore, pp. 140–1.

(382) *GE*, p. 393; *AG*, p. 59.

(383) Moore, p. 123.

(384) *Sp*, p. 172.

(385) Cf. note *380*, especially the "material factors" of Usnadze.

(386) Cf. G. von Wartensleben, *Die Christliche Persönlichkeit im Idealbild*. Kempten and München, 1914, pp. 2–3 (note).

(387) Stumpf, *SpE*, pp. 6 f.

(388) Sully, p. 30.

(389) *Op. cit.* (cf. note *302*), pp. 600 f.; cf. also Bühler, *GE*, pp. 425 f.

(390) Compayré, II, pp. 41–2.

(391) *GE*, p. 234.

(392) *GE*, pp. 415 f.; *AG*, pp. 143 f.

(393) Cf. the article cited in note *397*, and Piaget's *LP* and *JR*, especially the conclusion of *JR*, pp. 263 f., where he gives a concise, factual, and theoretical summary of the entire work.

(394) *Sp*, p. 212; *PC*, pp. 377 f.; cf. also *EA*, pp. 9, 16.

(395) *GE*, p. 137.

(396) *GE*, p. 137.

(397) Cf. C. Guillet, "Retentiveness in Child and Adult," *American Journal of Psychology*, 1909, *20*, pp. 318 f.

(398) Cl. and W. Stern, *Sp*, p. 163. Cf. the following with *ibid.*, pp. 164 f.

(399) *CP*, p. 132.

(400) *CP*, pp. 292 f.

(401) Cf. J. Piaget, "Les traits principaux de la logique de l'enfant," *Journal de Psychologie*, 1924, *21*, pp. 48 f. The citation from Claparède on p. 75 comes from the French ed., p. 522.

(402) Lévy-Bruhl, II, pp. 144 f.

(403) M. Wertheimer, "Über das Denken der Naturvölker. I. Zahlen und Zahlgebilde," *Zeitschr. f. Psychol.*, *60*, 1912. Now reprinted in the volume referred to in note *101*.

(404) *Op. cit.*, p. 329.

(405) *GE*, pp. 205 f.

(406) *Sp*, p. 250.

(407) Wertheimer, *op. cit.*, p. 327.

(408) Stern, *Sp*, pp. 248 f. The procedure is quite analogous in other languages.

(409) *Sp*, p. 251; *PC*, p. 381.

(410) Either achievement can be disturbed independently of the other. A case in which the ability to form groups was entirely lacking has been carefully studied by W. Benary, who reports his results, together with a discussion of all the theoretical possibilities, in "Studien zur Untersuchung der Intelligenz bei einem Fall von Seelenblindheit" ("Psychol. Anal," etc., edited by Gelb and Goldstein, VIII), *Psychologische Forschung*, 1922, *2*, pp. 209 f. Concerning

the experimental investigation of "numbering" by children, cf. Stern, *PC*, pp. 413 f.

NOTES TO CHAPTER VI

(411) Cf. the following with Ed. Spranger's *Psychologie des Jugendalters*, Leipzig, 1924, pp. 32 f.

(412) Cf. Bühler, *GE*, p. 325.

(413) *Op. cit.*, pp. 38, 47.

(414) Cf. *Paideuma, loc. cit.*, p. 59.

(415) Sully, p. 30.

(416) Lévy-Bruhl, Introduction and Chapter I, K. Th. Preuss., "Die geistige Kultur der Naturvölker," *Aus Natur. u. Geisteswelt*, Nr. 452. Leipzig and Berlin, 1914. See also the discussion of the child's animism in Piaget, *RM*, pp. 157 f.

(417) Cf. Volkelt, pp. 26, 43.

(418) *GE*, p. 389.

(419) Cf. Piaget, *JR*, pp. 335 f.

(420) Cf. Preyer, II, pp. 197 f.; also the reports on chimpanzees which Köhler has recently published, showing how they behave before a mirror. ("Zur Psychologie der Schimpansen," *Psychol. Forschungen*, 1, 1921, pp. 35 f. English translation in *The Mentality of Apes*, pp. 317 f.).

(421) *Op. cit.*, pp. 96, 80.

(422) *PC*, p. 273.

(423) Cf. Lévy-Bruhl, II, p. 124.

(424) *Op. cit.* in note *401*, p. 53.

(425) "An Investigation of the Development of the Sentence and the Extent of the Vocabulary in Young Children," *Univ. of Iowa Studies, Studies in Child Welfare*, 1926, *3*.

(426) Reported by Lévy-Bruhl, pp. 59, 62, 124 f. The citation is from p. 127. Cf. Volkelt, *Kongressbericht*, *loc. cit.*, pp. 110 f.

(427) *PM*, p. 387 f. This whole book is pertinent to the questions under discussion, pp. 380 f., 385 f.; cf. further *PA*, pp. 287 f.; *SK*, p. 205; Bühler, *GE*, p. 326; Stern, *PC*, pp. 273 f.

(428) *Op. cit.* in note *401*, p. 93.

(429) *Op. cit.* in note *401*, pp. 51 f., 91 f., and *JR*, p. 325.

(430) *CP*, pp. 142 f., 274 f.; *RM*, 112 f.

(431) Claparède is tireless in emphasizing this point and „its pedagogical implications, which he defends against preconceptions of any sort (cf., for instance, French ed., pp. 492 f.).

(432) *Op. cit.*, p. 48.

(433) *SpE*, pp. 18, 22, 16.

(434) Else Roloff, "Vom religiösen Leben der Kinder," *Arch. f. relig. Psychol.*, 1921, *2–3*, pp. 194 f.

(435) *PM*, p. 387.

(436) As Groos remarks, Spencer had already expressed the same idea, though Groos came to his theory independently of Spencer. Cf. *PM*, pp. 374 f.; *SK*, pp. 70 f.

(437) Groos, *PA*, pp. 1 f.; *PM*, pp. 361 f.; *SK*, pp. 64 f.

(438) *GE*, pp. 454 f.

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